AMERICAN NEUROLOGICAL ASSOCIATION.

TENTH ANNUAL MEETING.

First day, afternoon session.

The American Neurological Association convened at the New York Academy of Medicine, June 18, 1884. In the absence of the retiring President, Dr. Robert T. Edes, of Boston, Dr. William J. Morton, of New York, the retiring Vice-President, called the Association to order at 2:30 P.M., and after the following remarks introduced the President elect, Dr. Isaac Ott, of Easton, Pa.

Present—Drs. W. J. Morton, Ott, Bannister, Rockwell, Amidon, Birdsall, W. A. Hammond, G. M. Hammond, Spitzka, Wilder, Weber, and Gibney.

REMARKS.

In the absence of the retiring President it devolves upon me as Vice-President to call this the tenth annual meeting of the American Neurological Association to order.

In a letter from Dr. Edes to me he expresses his extreme regret at not being able to be present this year to furnish a paper, and wishes for the Association a pleasant and a profitable meeting. I cannot fill the position our retiring President would have filled in making his parting remarks. I will say but a very few words.

It is not for those who are just stepping off the official board to be seen and heard. We are of the past and must step aside, and hand over to others the insignia of office. For us there is only left a retrospect and warm words of cheer for the future. In looking back at our year just past we may justly say that the Association has taken no backward step. More than the usual annual accession of active members have joined or are about to join our ranks. The published report of our proceedings of last year was prepared with great care and has been published entire or in part far and wide, and in many languages.

And the glance backward at the cause which we represent is full of suggestion, guidance, and assurance. Ten years ago, when this Association was first organized, none of us would have ventured to say that neurological specialism would have assumed the stalwart proportions that it to-day possesses. The special workers in neurology were few, were isolated, but they were devoted. They were men who saw plainly that the whole of medicine, its etiology, its pathology, its symptomatology, and its therapeutics could not be grasped by the ordinary human mind, especially when trammelled by the wearying and anxious routine of daily practice. In the marvellous onward strides of medical science of to-day division of labor—even of intellectual labor—became a necessity. And thus sprung up the specialist—not alone the specialist in neurology, but the specialist in many other branches. And yet to-day there are many who speak disparagingly of specialism in medicine. To these a word:

The growth of neuro-pathology has been rapid and strong. Its followers, while clinging closely to their line of study and research, have thrown back into the general fund of medical knowledge more, in my opinion, than any other separated branch of medical workers. General medicine and surgery are to-day under constant obligation to the neuro-pathologist. The general practitioner no longer dares to speak of a paraplegia, but is tolerably wise in specifying it as a lateral or posterior sclerosis, or a transverse myelitis, and so on. The surgeon, by reason of the facts of cerebral and cranial localization finds himself forced to the intricate study of cerebral centres and tracts, to say nothing of nerve-suture, nerve-section, and nerve-stretching. Neurological nomenclature, formally confined to few, is now becoming medical vernacular.

This state of things is creditable to the neurologist. It justifies him in his subdivision of labor. Could he do more, could he do an equal amount of work in all the branches of medicine, he doubtless would. As I have already said, in an address before the N. Y. Neurological Society, in referring to the subject of neurological specialism: "The very attempt to keep up in the race of accumulating knowledge begets hypocrisy and discouragement. Specialism is, then, the substitution of precision for vagueness—of a concrete differential diagnosis for an abstract supposition; it is the forced acknowledgment that the average human mind cannot practically grasp all of medical science; it is, in short, the protest of knowledge against ignorance, and it now becomes, indeed, a matter of conscience to specialize medical labors according to the tasks or talents of the physician."

The march of medical events has then fully justified the organization and continued life of this American Neurological Association. With this brief retrospect I turn to the present.

It now becomes my agreable duty to introduce the President-Elect, Dr. Isaac Ott, of Easton, Pa.—No words are needed from me. His works have already spoken for him, and now as your President he will speak for himself.

THE PRESIDENT'S ADDRESS.

Gentlemen: At our last meeting it was with considerable surprise that I learned you had elected me to the principal office of this society. It is not proper for me to dispute the fitness of your choice, hence I thank you for the honor you have seen fit to confer upon me.

As it is the custom of this Association to demand an address, it has seemed to me that a consideration of the paths of the various fibres in the spinal cord would be a subject worty of consideration. My remarks, as is customary with me, will be brief. It is now about seventy years ago that Charles Bell announced that the anterior roots of the spinal cord were motor and the posterior sensory. During this period many thousands of experiments have been made upon the paths of conduction of motion and sensation in the spinal cord, with a gradual increase of knowledge. The physiology of the spinal cord, as taught to-day, is mainly due to perfections in technique—that is, the use of immovable protecting knives, and the subsequent microscopic examination of the hardened cord, two points neglected by Profs. Schiff and Brown-Séquard. I know of no more deceptive thing than to assert the extent of division of the spinal cord, without a careful microscopic examination. In regard to the paths of conduction, various opinions were held by different observers up to 1840, when Longet made numerous experiments upon the subject and arrived at the conclusion that the anterior columns were motor and the posterior sensory.

Brown-Séquard also made an extended series of experiments and believed that the transmission of sensory impressions in the spinal cord takes place chiefly through the gray matter, and partly through the anterior columns, that the voluntary impressions in the upper part of the cervical cord run in the lateral columns and in the gray matter, between them and the anterior columns; that no tactile sensations ascend the posterior columns to the brain. As to the kind of sensations conveyed upward, he made at least seven. These views, it is proper to state, have been somewhat modified by his studies upon inhibition. Prof. Schiff's idea of the spinal cord is that the posterior columns conduct tactile impressions, that the gray matter conducts in all directions afferent impulses which give rise to affections of the general sensibility, and such afferent impulses as are paths of reflex action. His theory of the spinal cord is as follows: if at any segment of the spinal cord the white matter is completely divided, and the gray is the only conducting link, then tactile and voluntary movements are absent in the parts below the injury, though violent stimulation of those parts will give rise to pain) and reflex actions in them may be induced by stimulation of the parts above the injury. If the gray matter only is destroyed and the white intact, voluntary and tactile sensations remain in the parts below the injury, but violent stimuli cause no pain, and reflex actions cannot be induced in them by stimulation of the parts above the lesion. I mention these various views with which you are perfectly familiar to show that the facts put forth are greatly in need of correction. Perhaps the first observer to direct attention to the importance of the lateral columns in the conduction of voluntary motion was Türck, of Vienna, in his studies upon descending degeneration. It was reserved for Ludwig, by his experiments upon rabbits, to bring the whole subject into the area of renewed observation. He and his pupils first proved that, upon irritating the scrotic nerve, the stimuli were conveyed by the lateral columns to the monarchical vaso-motor centre, causing a rise of blood-pressure. By the same means he proved that the vaso-motor fibres ran in the lateral columns. Prof. Schiff contends that the fibres here concerned are purely vaso-motor reflex and not sensory, which may or may not be true. This objection will not be of any weight when we come to examine the experiments of Ludwig and Woroscheloff, which were made by means of the apparatus which allows the protecting knife to remain immovable no matter what the position of the animal at the time of cross-section. They proved that, for the lumbar cord of the rabbit, sensory and motor fibres ran in the lateral column. Afterward, in conjunction with Dr. R. R. Smith, I proved the same statement to be true for the cervical segment of the spinal cord of the rabbit. To these experiments the objection was rightly made that the animal was not permitted to live for a considerable time after the operation. But experiments of mine on kittens who sustained their injuries for several weeks, proved that in addition to the lateral columns, the anterior contained voluntary motor fibres. Herr Weiss has also made a series of experiments upon dogs at the junction of the dorsal and lumbar segments, and permitted the animals to live for a considerable time. He arrived at the same conclusion as Woroscheloff, that the sensory and motor fibres run in the lateral columns, and that the gray matter does not directly conduct. Herr Kusmin also made several experiments upon dogs, and found that the gray matter did not conduct longitudinally. According to him the lateral columns are the important conductors of sensory and motor fibres, that the posterior columns conduct sensibility and the anterior columns motion. The only recent observer who still defends the theory that the motor and sensory fibres do not lie mainly in the lateral columns is K. Osawa, who worked in Prof. Goltz' laboratory. His theory is that section of both lateral columns diminishes the sensibility but not motility. He operated upon dogs, lifting the spinal cord slightly out of the spinal canal by a curved needle and then making a section. Whilst the original work is out of print, I see no reference in abstracts to a careful microscopic examination of the spinal cord. Brown-Séquard states that the smallest quantity of gray matter suffices for the purpose of conduction, and I wish to state that in kittens I have seen a layer of white matter of the lateral column, about the thickness of the spinal dura mater, suffice to conduct sensation and motion in such a complete way that no marked difference was seen except the ataxia. All these experiments go to show that the lateral columns are the principal conductors of sensation and motion, that the gray matter does not directly participate, that the anterior columns conduct voluntary motion. As regards the posterior columns I can substantiate Prof. Schiff's assertion that, after bleeding a rabbit and dividing the whole cord except the posterior columns, impressions of touch are conveyed by the posterior columns. That tactile impulses also pass up the lateral columns is also true, as can be shown by the pupil test, which takes place through the lateral columns. These are the chief points at which I have arrived after considerable experimentation upon the spinal cord. I have not referred to the recent works of comparative anatomists, but in the main they are in accord with these views.

I also should like to state that decussation of fibres in the central nervous system is quite extensive ; thus the motor, sensory, vaso-motor, sudorific, and inhibitory, the two latter running in the lateral columns, all cross over each other.

The next order of business was the reading of the minutes of the last annual meeting. Dr. MORTON moved that as they had been published, the reading of the minutes be dispensed with. Carried.

The Council reported, recommending election to active membership: Dr. Sarah J. McNutt, of New York, endorsed by Dr. E. C. Spitzka; and Dr. G. Betton Massey, of Philadelphia, endorsed by Dr. C. K. Mills.

Dr. W. A. HAMMOND asked—How is it that these candidates are recommended for membership and have not been proposed in open meeting?

Dr. AMIDON.—We did not find any by-law stating that it was necessary that candidates should be proposed in open meeting.

Dr. HAMMOND.—I would like to ask why it is, then, that Dr. Corning's name was not reported upon, whom I nominated last year.

Dr. MORTON.—That can be answered by the explanation that the Council were not able to agree on the name of Dr. Corning, and that the by-law is susceptible of a double interpretation on the point as to whether it is necessary to make a nomination in open meeting.

Dr. SPITZKA.—I would ask, what is to be gained by the last procedure. If the Council is unable to agree, the nominator has another privilege, and that is, to nominate and let the nomination go before another Council, and there is but little advantage in the change.

Dr. HAMMOND.—I would like to inquire what the reason was that Dr. Corning's name was not reported upon. Was not the paper which he submitted a suitable one?

Dr. AMIDON.—The reason why his name was not reported upon was because Dr. Morton voted aye, and I voted nay.

Dr. HAMMOND.—Did you read the paper?

Dr. AMIDON.—No, sir.

Dr. HAMMOND.—Then you voted prejudiced against the candidate, and I make that as an open charge in the Society.

Dr. SPITZKA.—I think there is a way of getting around that point without making open charges in the Society. The Council in the past has been in the habit of reporting upon those candidates who are reported upon favorably, and without mentioning the names of those who receive the unfavorable action of the Council. Now, I submit that it would be well to report upon all candidates—those who are and those who are not rejected by the Council,—and in that way the Society will have the opportunity to accept or not to accept the report of the Council.

Dr. HAMMOND.—It is no matter about that. The other candidates have been recommended, and it is supposed that the papers were read and reported upon. In this case the paper was not read, and his name is not returned favorably. Now, I say that this is a disgraceful act in the report of the Council.

Dr. MORTON.—I must say a word in defence of the Council. In an informal discussion on the candidate referred to by Dr. Hammond, I found that we could not agree, and therefore took no action, and since the case could not be acted upon, it was allowed to stand in *statu quo*, leaving it to be brought up at the next meeting of the Council. I do not think Dr. Hammond should find fault with that. With regard to not reading the paper, I beg to say that Dr. Amidon is not in fault with regard to that. Section five of the by-laws says that all papers shall be forwarded not later than one month before the annual meeting.

Dr. HAMMOND.—That does not apply to candidates' papers. Besides, it had been always customary to nominate every member in open meeting, and therefore the action of the Council in these other cases is irregular.

Dr. MORTON.—I think the record will show that it has been done previously, since neither the by-laws nor constitution make any mention of the manner in which it should be done.

Dr. WEBER.—I would ask how many members of the Council of the American Neurological Association make a quorum?

The PRESIDENT.—There is no statement in the by-laws on that point. There are five members in the Council.

Dr. WEBER.—I take it for granted, then, that a Council meeting of two was not a meeting of the Council, and therefore that this report is null and void.

Dr. MORTON.—There is no specification in the by-laws as to how many members of the Council constitute a quorum. It simply says that the Council shall do so and so.

Dr. HAMMOND.—But in the rules of all deliberative bodies when no quorum is mentioned, a majority of the entire number of members shall constitute a quorum.

Dr. SPITZKA.—I move, as there seems to be some question as to the legality of the meeting of the Council, that the report be laid upon the table, that the by-laws be suspended, and that we proceed to ballot for the candidates before the Society.

Dr. WILDER.—I would suggest that the motion be divided.

Dr. SPITZKA.—I would move, then, that the by-laws be suspended, and that the Society proceed to vote upon the reception of the candidates.

Dr. WEBER.—How shall we know how to vote, not having read the papers of the candidates? nor have we any information concerning their contents.

Dr. SPITZKA.—I withdraw my motion.

Dr. WEBER.—I now move to lay the report of the Council upon the table. The motion was seconded by Dr. Morton, and carried.

Dr. WEBER.—I now move that the papers be referred back to the Council to be read by the majority of the Council, and that a report of the Council be submitted thereon to-morrow.

Dr. AMIDON.—I do not think such a motion is necessary, The Council is supposed to meet before every meeting, and report with regard to business, etc., and if a quorum is present it will meet and report this evening.

Dr. HAMMOND.—The proper motion to make would be to not receive the report. It is not before the Society, as the nominations should have been made in open meeting before any action was taken with regard to them.

Dr. MORTON.—There is now a regularly elected Council in existence, and I do not see why this matter does not drift into the regular order of business.

The Society then proceeded to the regular order of business.

Dr. AMIDON read the report of the Treasurer, which, upon motion, was accepted and adopted.

NOMINATION OF CANDIDATES.

Dr. R. W. AMIDON nominated Dr. Sarah J. Mc Nutt, of New York. The nomination was seconded by Dr. W. A. Hammond.

Dr. W. A. HAMMOND nominated Dr. J. Leonard Corning, of New York. The nomination was seconded by Dr. Ott.

Dr. W. R. BIRDSALL nominated Dr. George W. Jacoby, of New York. The nomination was seconded by Dr. Spitzka.

Dr. E. C. SPITZKA nominated Dr. G. Betton Massey, of Philadelphia. The nomination was seconded by Dr. W. A. Hammond.

Dr. W. J. MORTON nominated for associate member S. F. Danillo, M.D., Privat-Docent at the Imperial Academy of Medicine, Physician to the St. Petersburg Clinic, formerly in the laboratories of both Vulpian and Charcot. The nomination was seconded by Dr. Weber. Dr. Morton, in connection with the nomination, submitted to the Association several monographs written by Dr. Danillo.

On motion the nomination of candidates was closed.

NOMINATION OF OFFICERS.

For President, Dr. R. W. AMIDON nominated Dr. Burt G. Wilder, of Ithaca.

Dr. W. A. HAMMOND nominated Dr. C. K. Mills, of Philadelphia.

On motion the nominations for President were closed.

For Vice-President, Dr. W. A. HAMMOND nominated Dr. Leonard Weber, of New York.

On motion the nominations for Vice-President were closed.

For Secretary and Treasurer, Dr. E. C. SPITZKA nominated Dr. G. M. Hammond, of New York.

Dr. W. R. BIRDSALL nominated Dr. R. W. Amidon, of New York.

On motion the nominations for Secretary and Treasurer were closed.

For Councillors, Dr. R. W. AMIDON nominated Dr. W. R. Birdsall, of New York, and Dr. W. J. Morton, of New York.

Dr. W. R. BIRDSALL nominated Dr. E. C. Seguin, of New York.

On motion the nominations were closed.

The President appointed Drs. W. J. Morton, of New York, and H. M. Bannister, of Chicago, Tellers, and, as a result of balloting the following officers were elected:

President, Dr. Burt G. Wilder, of Ithaca, New York.

Vice-President, Dr. Leonard Weber, of New York.

Secretary and Treasurer, Dr. G. M. Hammond, of New York.

Councillors: Drs. W. R. Birdsall and W. J. Morton, of New York.

The Association then proceeded with its scientific work.

The first communication was by Dr. B. G. Wilder, of Ithaca. "Exhibition of Preparations Illustrating (*a*) the Existence and Circumscription of the Portæ (Foramina Monroi) in the Adult Human Brain; (*b*) the Presence of the Crista Fornicis in Fœtal and New-Born Human Brains; (*c*) Two Additional Cases of Absence of the Callosum in the Domestic Cat; (*d*) the Covering of the Cerebellum by the Cerebrum in a Young Chimpanzee whose Brain was Hardened within the Skull.

Remarks on Dr. Wilder's Communication.

Dr. R. W. AMIDON, of New York, exhibited a human brain wanting the callosum. The specimen had been loaned to him by Dr. Frank Ferguson, of New York, who had already presented it to the New York Pathological Society, and of which a complete history would be subsequently published in the journal, edited by Drs. Mc Bride and Spitzka.

Dr. E. C. SPITZKA said with regard to the question of the cerebrum overlapping the cerebellum, the specimen presented by Dr. Wilder illustrated how important mere morphological comparisons were in reaching definite conclusions. It was an error to suppose that preponderance of the cerebrum over the cerebellum was a sign of high development. On the contrary, the cerebellum being an important midway station, as it were, through which all the fibres that originated in the spinal cord traversed to be transported toward the cerebrum, it stood to reason if there was any intimate relation between the two bodies, both must progress more or less equally. As we proceed from the lower to the higher forms of animal life, it is not in the fact of the cerebrum or of the cerebellum becoming more developed, but that in the cerebrum we find differences in size and evidence of hemispherical preponderance over the vermiform lobe, so that when we proceed from the anthropoid apes upward we still find overlapping, and that the lesser overlapping is really a sign of a higher development. In the lower monkeys the overlapping is remarkable, and reminds one of the overlapping seen in the brain of the human embryo, so that when you separate the hemispheres when the brain is very small you will see the corpora quadrigemina develop immediately in front of you. The higher the development the less the overlapping is quite contrary to the conclusions formed by Dr. Chapman, who started out with the view that the cerebellum was not overlapped by the cerebrum, and in defence of that theory he presented a series of anatomical specimens, but the real fact was that he allowed the brains which he prepared to lie upon the bottom of the glass vessel and flatten out, as has already been suggested by Dr. Wilder. Dr. Spitzka had seen more than one human brain removed by the ordinary methods which showed variation not only with reference to the degree of overlapping, but showed the cerebellum to be already to one side, and perhaps the crura broken, by the straining which had been placed upon the specimen when removing it.

Dr. WILDER thought that a vast amount of printer's ink, time, and some temper would have been saved had certain anatomists undertaken to be sure that their brain specimens were of normal shape before undertaking to publish any figures or description, much less draw any deduction thereon. If he were to urge one thing more than another in the study of comparative anatomy it would be to urge that proper methods be taken in the removal and subsequent preparation of these anthropoid brains, and it was certain that had such methods been observed neither Dr. Chapman nor any one else would have advanced any such notion as had been advanced, and therefore controversy could have been avoided.

Dr. SPITZKA directed attention to the somewhat sensational contribution made by Benedikt, of Vienna, who had stated that in criminals the cerebellum is uncovered by the cerebrum. In a subsequent investigation and discussion of this point by Meynert and H——, they showed that the lack of overlapping was due to precisely such methods of preparation as Dr. Chapman had resorted to in his investigations of the brain of the chimpanzee, laying the brain unsupported flat upon its base at the bottom of a vessel.

Dr. WILDER thought that without sacrificing the skull, the brain, if it is removed in the dura mater, could be well preserved, as when the dura and the brain become filled with alcohol the dura becomes quite tense and the brain assumes nearly the form it had in the skull. The brain so swells that it fills the dura completely, and is tense like a ball, and there is but little, if any, difference in its shape as compared with what it had during the life of the individual. His method of removing the brain in the dura mater was to open the skull as low as possible, using nippers in the skull and removing the dura with the spatula, according to the directions which he suggested at the last meeting of the Association.

On account of the absence of several members who were expected to contribute statistics on the subject of tabes dorsalis and syphilis, the reading of the paper by Dr. E. C. Seguin, of New York, was postponed until some future session of the present annual meeting.

The Society then adjourned to meet at 8:30 P.M.

First day, evening session.

The Association was called to order at 8:30 P.M. by the President.

Present—Drs. Ott, Amidon, Wilder, Dana, Spitzka, Rockwell, Birdsall, Webber.

The minutes of the afternoon session were read and adopted.

The Council reported favorably on the nominations for active membership of Drs. Sarah J. McNutt and George W. Jacoby, of New York, and G. Betton Massey, of Philadelphia.

Dr. SPITZKA moved that the consideration of the report be postponed until to-morrow.

Dr. AMIDON.—That involves a change in the by-laws.

Dr. SPITZKA moved that the by-laws be suspended. The motion was seconded by Dr. Rockwell. Dr. Spitzka thought it well that the matter should be brought before the Society as a whole, in view of the palpable violation of the by-laws with regard to one candidate, as shown by the proceedings of the afternoon session.

Dr. WILDER.—As profound as my respect for Dr. Spitzka's judgment is, and as ardent as is my wish to accede to what he desires, I think the motion to postpone involves the more serious objection of attempting to overrule what I prefer to regard as the deliberate action of the part of the Association elected to transact certain business for us and to preside over us. I should think we would be making a very dangerous precedent, indeed, if we adopt the course proposed by Dr. Spitzka.

Dr. SPITZKA.—I fully coincide with the views just presented by Dr. Wilder; but, at the same time, I think it was made so painfully evident to a large number of the members of the Society at the afternoon session that the Council did not do its duty, but flagrantly violated its duty,—did not even condescend to consider the paper of the candidate,—that I have made the motion simply as a matter of principle. If it is shown, as was shown so glaringly this afternoon, that personal pique, and feeling of a lower character even, animated the Council, I think it is high time that we should know it, and settle it in a way which will prevent, if possible, any thing of the kind ever occurring again. My object in moving to postpone the report is not to place myself in a position of antagonism to the report, but it is made in order to allow the Association as a whole to pass upon a very important matter indeed. If the Association pass over the matter as it stands now, however, it can be brought up in another form.

Dr. WEBBER, of Boston, asked whether, when the subject was referred back to the Council, there was an understanding as to what time the report should be made.

The PRESIDENT.—I do not know that any definite time was stated at which the Council should report.

Dr. WEBBER.—Was there any expectation that it would report to-night?

Dr. SPITZKA.—I think that they were to report to-morrow. I think that was the idea in referring the matter back to the Council.

Dr. WEBBER.—If there was any definite understanding concerning the time when the Council should report, that motion should be considered; but if there was no special understanding on that point, there has been time sufficient for the Council to reconsider their views, and be ready for their report, and those who were interested this afternoon should be here this evening, if they wish to vote upon the subject. I do not see any reason why there should be any postponement of action on the report of the Council.

Dr. AMIDON.—The report of the Council this afternoon was laid upon the table because it was thought to be illegally made, from the fact that of the five members of the Council only two members were present at the time the action of the Council was taken which formed the basis of the report. After the afternoon session the President called a meeting of the Council, and four members were present, and the Council arrived at the report presented, and that, it seems to me, should be final.

Dr. SPITZKA.—Dr. Amidon has forgotten to say that one of the gentlemen of the previous Council had neglected to read the paper of the candidate, and that was one of the material defects upon which I based my remarks a while ago.

Dr. WILDER.—Did the other members of the Committee read the paper?

Dr. AMIDON.—Not that I know of.

Dr. WILDER.—I took it for granted that one member of the Council read the paper, and that the other trusted to him in the matter. It is not always possible, or at least practicable, for every one of the members of the Council to read every paper; and, with my feeling of regret that what did occur this afternoon should have occurred, I still feel that we have elected a President and have associated with him other officers and two members constituting the Council, and in this report three other members besides the President are represented. Therefore, unless we are prepared to accuse them of injustice in the face of a serious and sober suggestion made this afternoon,—unless we are prepared to do that, and declare them lowered in our estimation, we should uphold their action in the report for this evening.

Dr. SPITZKA.—I technically agree with Professor Wilder. I did not intend to throw any reflection upon the Council which met this afternoon. I think we are bound by their action. But I do not think we should be hasty in adopting their report when there are so many other members personally interested in the matter, as I am not. It is perfectly indifferent to me as to whether a given candidate is elected or not, but the glaring existence, this afternoon, of something which I hesitate to characterize, and which I fear has been infused into the second meeting of the Council, has induced me to offer the motion which I have made. I wish, however, to withdraw my motion, and will state that I wish to give notice of a proposed change in the by-laws which shall come up for consideration next year.

With the consent of Dr. Rockwell, who seconded Dr. Spitzka's motion, the motion was withdrawn.

Dr. WILDER moved that the report of the Council be accepted and adopted.

Dr. SPITZKA moved that so much of the report of the Council be adopted as related to the recommendation of three candidates for active membership—Drs. McNutt, Jacoby, and Massey.

Dr. AMIDON.—I ask for information if Dr. Spitzka's motion does not involve a suspension of the by-laws.

Dr. SPITZKA.—There are negative features in the report of the Council. It is a notorious fact that other candidates were proposed, and it is also known that a contest arose about that very question, and in order to give a fair opportunity for discussion, the motion which I made allows the negative portion of the report to be taken up at any future time, while it allows the candidates reported upon favorably to be elected at once.

Dr. WEBBER.—I do not understand that there is any negative portion in the report. The report is positive, and recommends to the election of active membership all three of the candidates, and when we have balloted upon these candidates the matter is ended, and there are no negative features.

Dr. SPITZKA.—Then I call for a complete report of the Council.

Dr. BIRDSALL.—That would require another meeting of the Council.

Dr. WILDER.—I do not think the gentleman has any right to force a negative statement upon the Association.

Dr. SPITZKA.—The matter, as referred back to the Council, and in its hands, was this: there were four candidates to be reported upon. Three have been reported upon, and the other has not been reported upon, and that is the negative portion of the report. Why is this business done in secrecy, and not above board? That is what we wish to know, and that is the only object I had in making the motion which I do with reference to the report of the Council.

Dr. WILDER.—I must be allowed to say that I have no such information, nor have I any such suspicion. I could not conceive that the gentleman whom I had in mind can possibly be the candidate referred to from the fact that he has been present this afternoon and is present this evening.

The amendment offered by Dr. Spitzka was lost.

The original motion, as offered by Dr. Wilder, that the report of the Council be accepted and adopted, was carried.

The President appointed Drs. Dana and Rockwell tellers, and the Association proceeded to ballot for candidates for active membership reported favorably by the Council, and they were all unanimously elected.

THE WILLIAM A. HAMMOND PRIZE.

Dr. AMIDON read the report of the committee—Drs. F. T. Miles, of Baltimore, J. S. Jewell, of Chicago, and E. C. Seguin, of New York,—announcing that no essay of sufficient merit had been presented for the prize, and that consequently it was not awarded.

Dr. WILDER moved that the report of the committee be accepted and adopted. Carried.

MISCELLANEOUS BUSINESS.

Dr. E. C. SPITZKA, of New York, then gave formal notice of a proposed amendment to the by-laws, adding a clause which provides for an executive session in which all business of the Association, such as the election of members, officers, etc., shall be transacted.

SCIENTIFIC WORK.

Dr. B. G. WILDER, of Ithaca, then read a paper on "Macroscopic Encephalic Nomenclature."

Like other studies of the subject by the same anatomist, the general purpose of this paper is to facilitate the advancement and dissemination of our knowledge of the coarse anatomy of the brain by reducing the difficulties now presented by the undigested mass of ponderous, polynomial terms now employed.

Professor Wilder urges the adoption of *technical*, *brief*, *monomial* terms, most of which may be had by subjecting those in common use to *abbreviation*, *translation*, and *combination*.

He also suggested that more frequent and systematic use be made of the various methods of classifying the encephalic parts, and thus their names, in accordance with the segmentation of the organ, its constitution by cavities and parietes, and the division of the latter into commissures, etc.; and especially the recognition of certain parts as *hypertrophied*, *atrophied*, and *marginal* (*tænia*, *fimbria*, *crista*, *habena*,) etc.

Specific recommendations were (1) the substitution of *mesen*, *præn*, etc., as *abridgments* (not abbreviations) of *mesencephalon*, *præncephalon* and other segmental names;

(2) the designation of the ventricles by the Greek κοιλια (*cœlia*) in composition with the segmental prefixes, which would give *mesocœlia* (*mesocele*), etc. In this connection attention was called to the substantial coincidence of Prof. Wilder's series of cœlian names (first published in March, 1881) with the names more recently proposed by Prof. T. Jeffrey Parker, of New Zealand.

On motion by Dr. Amidon the discussion of the paper was postponed until Thursday afternoon.

Dr. A. D. ROCKWELL, of New York, then read a paper entitled " Tonic Spasm of the Diaphragm (?) "

The following case was still under observation, and had been for a long time. It was unique in character, and, for that reason, and because its pathology was so obscure, he had been constrained to present it to the Association in the hope of exciting an expression of opinion concerning its character.

Mrs. X——, thirty-five years of age, the mother of four children, began first to suffer from the paroxysms of distress about twelve years ago. They had remained substantially the same both in kind and severity as when they first appeared but their recurrence had been marked by uncertain and varying intervals. Before January, 1884, they seldom occurred more than once a year ; sometimes two and three years intervened between the attacks. From January to April she had two attacks, but for two months subsequently the attacks were so frequent that the count became lost. For some time they occurred every day, and occasionally twice a day, and were fast wearing the patient out. Latterly they had occurred between five and seven A.M. The patient is awakened from a sound sleep by a vague feeling of pressure and distress which is felt about the lower end of the sternum, over the epigastrium, and extends to the corresponding point in the back. It was described as if a crushing weight were being applied, as if the front and back were being pulled together. The recumbent position during the attack was impossible, and the patient leaned forward, moved about, or rested against some support, with short gasping inspirations, pallid countenance, and with an agonizing expression, until the paroxysm either subsided or an anæsthetic was administered. The pulse becomes weaker and somewhat less frequent, but is not at all irregular.

The attacks last from twenty minutes to three fourths of an hour, and leave the patient much prostrated, from which she rallies quickly, however, but a bruised sore feeling remains for many hours.

After the attacks became so frequent many efforts were made for her relief, with electricity in all its forms, together with internal treatment, and all unsuccessfully. Dr. Rockwell finally resorted to the actual cautery ; no attack had occurred for nearly one month, and the general health of the patient had improved steadily.

A difficulty, perhaps, in accepting the diagnosis, tonic spasm of the diaphragm, was found in the fact that there had never been, even during the severe attacks, any change in the contour of the external surface, no drawing in of the parts at the attachments of the diaphragm, nor projection of the epigastrium. Any attempt at swallowing, also, was always successful. Nevertheless, Dr. Rockwell was quite convinced that the symptoms were due to a spasmodic contraction of the diaphragm, notwithstanding the absence of certain objective symptoms, such as expansion and immobility of the lower half of the chest, projection of the epigastrium, closure of the œsophagus, which he had found emphasized in print and speech.

Remarks on Dr. Rockwell's Paper.

Dr. WILDER asked the reader of the paper if the pain described was similar to ordinary cramp in muscles.

Dr. ROCKWELL said that it was very similar.

Dr. DANA asked on what grounds angina pectoris was excluded.

Dr. ROCKWELL replied that the pain seemed to be rather low down for angina pectoris, in the epigastrum and extending through to the back to a point directly opposite. It seemed to him to be so entirely different from angina pectoris, of which he had seen a number of cases, that he had excluded that affection.

Dr. WEBBER asked where the cautery was applied.

Dr. ROCKWELL said it was applied all along the spine from the nape of the neck to the waist.

Dr. BIRDSALL asked Dr. Rockwell if he had satisfied himself with regard to the absence of hysteria and hepatic calculi.

Dr. ROCKWELL said that gall-stones had been excluded, that there was no evidence of hysteria in the case, but that the existence of hepatic calculi he had not considered very thoroughly.

Dr. DANA thought it would be physically impossible for the diaphragm to be in a state of tonic contraction without the abdominal muscles being tense or protruding, as this was one of the most noticeable phenomena produced in experiments on animals.

Dr. ROCKWELL said that that was just the point upon which he wished to get some light, and he was desirous to know exactly what the condition was. He made the title of his communication tonic spasm of the diaphragm, with an interrogation point. He was aware that spasm of the diaphragm was of very infrequent occurrence, yet while experimentation showed that there was change in the contour of the abdominal muscles when it existed, he was not prepared to say that slight spasm occurring in a very sensitive person might not take place without producing any noticeable change in the condition of the abdominal muscles.

Dr. SPITZKA asked Dr. Rockwell if there was any change in the action of the respiratory muscles.

Dr. ROCKWELL said that the only thing which he had noticed was a slight fluttering—only very slight—at the epigastrium. How much it had to do with the action of the respiratory muscles he was unable to say. The breathing of the patient was gasping and short, and was performed largely by the upper portion of the chest, but he did not notice any lateral expansion of the walls of the chest.

Dr. SPITZKA remarked that the objection urged against the existence of tonic contraction of the diaphragm because there was no change apparent in the abdominal muscles, might be overcome and explained if there was any change in the action of the respiratory muscles, especially if the tonic contraction of the diaphragm was only slight.

The Association then adjourned to meet on Thursday at 2 P.M.

Thursday, second day, afternoon session.

The Association was called to order at 2:30 P.M., by the President.

Present—Drs. Ott, Amidon, Gibney, Birdsall, W. A. Hammond, G. M. Hammond, Weber, Massey, Bannister, Wilder, Rockwell, Gray, Mills, McNutt, Dana, Webber, Bartholow, and Walton.

The minutes of the last meeting were read and approved.

Dr. W. A. HAMMOND, referring to the report of the committee on the prize essay, said that his intention was in offering a prize that the committee should have the power to reject all essays. The idea was not to offer a prize for a *paper* upon the functions of the optic thalmus, but a *prize* for original observations on the functions of the thalmus in man, with special reference to its perceptive faculties.

After some further explanation and discussion, Dr. WILDER moved to strike out of the minutes so much as related to a reference of the report back to the committee.

The Council reported favorably on the nomination of Dr. Danillo for associate member.

Dr. W. A. HAMMOND.—Has there been any report made upon the nomination of Dr. J. Leonard Corning—not upon his name, but upon his paper? As I understand it, the Council has no power to keep the Society from voting upon any candidate who may have been nominated in the regular order. If the Council has not made any report upon Dr. Corning's paper I move that the Society proceed to ballot upon the name of Dr. Corning for membership. I have taken pains myself to read his paper, and I am willing to say that any member of this Association might have been proud to have written it. It is true that it contains a great deal of material which has already been known, but he has brought out the matter in a different shape from that in which it has heretofore been presented, and he has also introduced more or less of original material. If there is no objection to the paper I should like to know what the objection is to Dr. Corning becoming a member. He has written a book, has contributed numerous articles to medical literature, and, as far as I know, is in good standing in the profession. If he has done any thing which is derogatory to the professional standard, I should like to know it so as to vote against him. But if there is no such evidence, I do not think any personal feeling should prevent the Society from acting upon the nomination. I therefore move that the Society proceed to vote upon the name of Dr. J. Leonard Corning for membership. There is nothing in the constitution to prohibit this action, even though the Council has not made any report. Even if his paper was not of the highest scientific order, that is not a reasonable objection against him. I think the Association has a right to vote upon the candidate, and I make that motion.

Dr. W. R. BIRDSALL.—I rise to second Dr. Hammond's motion for the reason that I wish to explain my position as a member of the Council, and as to what I understand to be the functions of the Council. I agree that the Association can overrule any action of the Council and accept any candidate upon whose paper the Council has made an unfavorable report, although such has not been the usual course. It seems to me that if the report of the Council is called for they have simply to report whether a candidate's paper was accepted or rejected, and the grounds on which the rejection was made, if made, it is not proper to furnish. The reason for rejection is often too delicate to be brought out, and the simple fact of rejection by the Council is evidence that they have some good reason for rejecting the candidate, and it must rest upon that ground, and the Association must rest content with the judgment of such men as they have elected to fill those positions and discharge the duties of Council. Still, as I have already said, the Asssociation has a right to elect any member without reference to the action of the Council.

The objection raised yesterday with regard to the question of quorum does not involve the meeting of the new Council, as four members out of the five constituting the Council were present. The objection raised that the paper should have been read was proper. I think no paper should be passed without sufficient study of its contents to enable the members of the Council to judge of its value, and as a member of the Council I know that this has been complied with.

Dr. HAMMOND.—My friend, Dr. Birdsall, whose motives no one who is at all acquainted with him will for a moment question, is entirely mistaken with regard to giving reasons for rejection of a candidate. Every committee of every society or organization is bound to give its reasons for its actions, and it does give its reasons, and the society has a right to call for them if it does not give them. This is a rule which obtains in the House of Congress and in all legislative bodies, and I think the Association has a right to insist upon this here. Dr. Corning's paper is a good paper, it is expressed in good English, has good spelling, good logic, and I do not see why it is not a paper which should entitle him to admission to the Association.

Dr. WEBBER, of Boston.—I think it would be better to postpone discussion upon this subject, as there are a good many present who are not members of the Association, and it would be far better to discuss it in some executive session.

Dr. HAMMOND.—But there are no provisions in the by-laws for an executive session.

Dr. WEBBER.—Then I move that the matter be laid upon the table until the scientific work of the Association is finished.

Dr. HAMMOND.—I hope that motion will not prevail as it is the easiest thing to come to a vote directly. All that is necessary to settle the question is to come to a vote, and no matter how it shall go I shall be satisfied. All I ask is a vote.

Dr. WEBER, of New York.—If the Council will not inform the Association why they rejected this candidate, it nevertheless will be understood that there is some good reason for its action, and if we go on as Dr. Hammond suggests, the great probability is that the candidate will be rejected, and therefore it will be better for the candidate not to have a vote upon the question at the present time.

Dr. HAMMOND.—Oh, no. That remains to be seen. It is not the function of the Council to inquire concerning the candidate. All they have to do is to decide on the scientific value of his paper. He may be a pickpocket as for that matter. All they have to do is to judge of the merits of his paper.

Dr. BIRDSALL.—I would ask for the reading of the by-laws regarding the function of the Council.

The secretary read the section of the by-laws relating to the function of the Council.

The Association then voted upon the motion to lay the subject upon the table. The motion was lost.

Dr. HAMMOND.—I now renew my motion, and move that the Association proceed to vote upon the name of Dr. Corning for active membership.

Dr. WEBBER, of Boston.—I do not know how it is in New York, but when there is a council of any society in Boston to decide upon a candidate, there is provision in some societies that if the council do not report favorably, the person proposing that candidate has the liberty to bring the name before the society; while in other societies the action of the council is absolute, and that finishes the matter. I do not know how it is here in this Society.

Dr. HAMMOND.—There is no provision in the constitution and by-laws with reference to the action of the Council of this Society being final.

The secretary read article eight of the constitution.

Dr. WEBBER.—It seems to me from the reading of the article of the constitution, that it provides for the election of a member independent of the report of the Council.

Dr. BIRDSALL.—I would like to say concerning the action of the Council, that the name of the candidate is presented before the Council and balloted upon, and that it is not necessary that the secretary of the Council should give any of the details of the conversation held between the members; but if the Society chooses, it can call for the result of the vote of the Council simply.

Dr. HAMMOND.—Then I should like to know how the vote stood; how many were present, and how the vote stood.

Dr. AMIDON said that there were present at the meeting of the Council Drs. Ott, Birdsall, Gibney, and himself, and that when the vote was taken three ballots were cast, of which two were against the candidate and one for him, and one member of the Council did not vote at all.

Dr. HAMMOND.—Did the president vote?

Dr. AMIDON.—I decline to answer.

DR. HAMMOND.—I ask, did the president vote?

The PRESIDENT.—No, sir.

Dr. HAMMOND.—The president would have voted aye, if he had voted, for he seconded the nomination of Dr. Corning, but he refrained from voting on grounds of delicacy. If he had voted, therefore, the vote would have been a tie-vote.

Dr. AMIDON.—I could believe that this action on the part of one, a distinguished member of this Association, was intended in good faith, did I not know the source from which it comes. But, unfortunately, being the subject of frequent personal abuse of like character from the same source, I will leave it. As I look upon it, the Council of the Association is the door-keeper of the Association, and is responsible for the members who are admited to this body. There are members now who abstain from the meetings of the Association, and who do not make any scientific contributions to the Society; and there are still other members who do not pay their annual dues, and have not paid them for a number of years. The Association has a world-wide reputation, and when such men as Charcot, Bernhardt, and Erb value honorary or associate membership, and when our membership is limited to fifty, I think it is the duty of the Council to be very conservative in the admission of new members. At the meeting of the Council where we acted upon the papers of candidates, I read Dr. Corning's paper while the other members listened to my reading, and then we simply voted, and of the four members who were present two voted against him, and one for him, and one did not vote at all.

Dr. HAMMOND.—All that would have come with better grace if the member had made his remarks before he cast his vote yesterday. It seems to me like the old fable of the wolf and the lamb.

Dr. WEBBER.—I do not suppose that any amount of talk will change the vote. I therefore move that the Association proceed to ballot. Carried.

The PRESIDENT appointed Drs. Rockwell and Mills tellers, who subsequently announced that the whole number of votes cast was eighteen, of which there were ten for the candidate and eight against him.

The PRESIDENT declared the candidate elected.

Remarks on Dr. Wilder's Paper on Encephalic Nomenclature.

Dr. C. L. DANA, of New York.—I am unable to speak upon the subject as a cerebral anatomist, but I have taken a great interest in the paper and the work which Dr. Wilder has undertaken. I have some interest in it as a teacher, and I can therefore offer some suggestions from that standpoint. As a teacher I think that most of the points advocated by Professor Wilder are admirable, and will eventually be adopted. There are certain special things which might be refused, but I think that every one will see certain advantages in the system proposed. But, at the same time, I think Prof. Wilder speaks too much from the standpoint of the anatomist or teacher of anatomy, forgetting that encephalic anatomy, or anatomy, is not of the same kind of pure science as certain other branches of natural science are. Encephalic anatomy is not only one of the sciences, but those who are engaged in its study and teaching must give terms which medical students and medical men can easily acquire. I know that Dr. Wilder has had this point in view all the time, but I think he has made some mistakes in some terms which he has introduced. I think he has made a mistake with regard to the terms diencephalon, epencephalon. Instead of these he proposes to call them dien, epen, proen, etc. In my opinion this change is unjustifiable and unnecessary because the most of medical students do not see the terms of encephalic nomenclature very often, and when they do see these names they will have to think two or three times before they know what it means, and there is no special harm in giving the entire word.

I should think also that Prof. Wilder has made a mistake in attempting to nickname different parts of the brain in this manner, because there are so few persons who read or see sufficient of the brain nomenclature to make it necessary to use nicknames.

I think also he has shown a tendency to increase the number of anatomical landmarks in the brain, as, for example, when he suggests the term "*ripa*" for a little artificial part of the brain, which is of rather small significance to say the most, and I do not see why he should add to the complexity of the subject by adding such terms.

I think that if he would devote the energy which he has manifested in getting up this system of nomenclature to simplifying that which already exists he would do a greater service.

I think he has made a mistake in trying to substitute certain names for common names, such as, for example, "alba" for "white matter of the brain"; "mesocœlia" for "aqueductus Sylvii," etc. I think there are several words which it is unnecessary to change simply for the sake of getting monosyllables instead of polysyllables. I also think that changing the word "ventricle" to "coelia" is not necessary, but I do not offer that as a positive objection to the substitution, only it has struck me thus far that it was not necessary to change the term ventricle, a word which has become a part of an anatomical thing, to the term cœlia.

He has also used the words "mesal" and "mesen." I think every one would agree that it is an admirable improvement. But I do not think it is necessary to carry it as far as he has done. He speaks of "dorsad," etc., and I think it is possible we could dispense with the termination "ad" and use the termination "ly," which is usually employed.

There is one further criticism which Dr. Wilder has in part already answered by one of the resolutions which he has offered. I think it is bold for any single man to attempt to reform cerebral anatomical nomenclature. It is impossible to form a nomenclature which will suit all anatomists, and if he had first endeavored to secure an international committee perhaps to discuss this subject he would have done a greater service, although every one must recognize the immense value of his services already. He has given an impulse to a reform which will not cease until something has been actually accomplished.

Dr. W. R. BIRDSALL of New York.—I hesitate to say a single word upon this subject, because few of us can discuss it properly. The subject is one which has been steadily elaborated by Prof. Wilder for a number of years, and such a task can be only undertaken by one almost constantly engaged in such work, and by one who gives almost his entire time to the subject of anatomical study and teaching. It seems to me that the subject, as embodied in the resolutions which he has offered, this Association should be proud to subscribe to, and to their author as a worker who has accomplished so much as Prof. Wilder has done by his labor. Perhaps the only qualification which I would make to them is with regard to the terms especially recommended in these resolutions, and that is simply because I do not feel competent to discuss the value of these terms. When we consider how slowly nomenclatures are built up, and the curious ways in which terms are added, it does indeed seem a marvellous task to attempt to reform it in any way, and any one who shows that he is competent to do such work by his knowledge of the parts to be named is certainly deserving of thanks for his labor in that direction. We are, as a rule, disposed to leave things alone, and object to the introduction of new terms. At the same time the confusion which now exists might be greatly simplified by making terms briefer, and restricting certain words and converting certain sentences into words. I can, on the other hand, see an objection to doing this, especially in nicknaming certain parts. If the term now in use means any thing it is of advantage to keep it whole ; if it is a description of a certain organ, or resemblance of that organ to an absolute or fancied body, it is of advantage to retain it, provided it is not too cumbersome. In making changes in the nomenclature, when we come to provide terms which will substitute a single word for a greater part of a sentence, we take the risk of losing the advantage of description given by the complete sentence, and as it occurs in general anatomy we lose the derivation of medical terms. It is true that so many of these terms have been made from fancied resemblances that in looking them over one would judge the poet had been over the ground instead of the scientist. In making changes which should bring about uniform nomenclature, we might take Chinese terms and apply them to certain tracts in the brain perhaps with advantage, because the student would know such and such terms indicated such and such organs or parts of organs, but it might be questionable whether such a system would possess any ultimate special advantages. But Prof. Wilder's method seems to have been, according to what has usually obtained, to retain the classical languages, Latin and Greek, and retain where possible a description which is in keeping with the real name and thing, as "mesencephalon," for example. But in writing I can see why the first part of the word alone might be of advantage. As far as speaking is concerned, I think it is about as well to use the whole term as to employ a part of it.

The objection which has been raised, that certain minute portions of the brain might as well not be named, is one which may or may not be open to objection. Many insignificant parts of the brain have been named, while on the other hand many parts have not been named which are very important parts when we consider their relation to embryology, etc.

From the standard of the evolutionist, who studies the part with reference to what it was in the embryo, some of these little points are of very great interest and well entitled to be named. It all depends upon the anatomist who claims whether they are of significance or not.

Whether a committee can do this work better than an individual is another important question. When we look at the names which have been brought forward by certain individuals, and see how they really occupy large territories in our nomenclature as compared with any work done by the action of societies, it is indeed a question whether an individual can not go far ahead of a united action in such a matter. When we consider what Owen has done, and later Huxley, how can we doubt that single individuals are capable of making up consistent nomenclatures.

I have no detailed criticism to make upon Dr. Wilder's nomenclature, and I think that in his hands more has been accomplished than would have been by many others, and that if we arrive at any conclusion, we should ask the action of other societies and learned bodies who will coöperate with us in an amendment or further suggestion in the adoption of any plan suggested. This line of work is now international, and embraces the entire world.

Dr. WILDER.—I wish, as I am obliged to leave in a few moments, to express my gratitude for the expressions of kindness with which my paper has been received, and I should be very glad to have time to comment upon a few points which have been raised, but this I must postpone, at least until next year. I would simply say that very many of these apparently new names are not new at all. If all anatomical works were read as thoroughly as are the Grays and Quains, it would be found that I am guilty of bringing only very few monsters into the work.

Dr. W. A. HAMMOND moved that the resolutions offered by Dr. Wilder be taken up as the first item in miscellaneous business to-morrow.

Dr. G. M. HAMMOND then read a paper entitled, " Can Locomotor Ataxia be Cured ? "

A few years ago when a member of the medical profession announced that he had cured a case of locomotor ataxia, his statement was not received with a great amount of interest. Belief in his views was not entertained for a moment. It was absurd even to talk about curing ataxia, and the physician was regarded as having made an inaccurate diagnosis, or else that an intermission of the disease had occurred at an opportune moment. The pathological condition found existing in the spinal cord in persons who have died while suffering from ataxia seemed to warrant the views held by a majority of physicians that a cure of this affection and all similar ones, such as sclerosis of different parts of the cord and brain, was impossible, but as time has passed reports of cases cured have come before the profession, many of them reported by men of such undoubted learning and high reputation that we can not afford to overlook them. The question, therefore, that arises is: Are these true cases of ataxia, or is there some other morbid condition of the spinal cord which when present gives rise to symptoms similar in every respect to those resulting from sclerosis of the posterior columns? Dr. Hammond had collected a few cases of undoubted cure of what was supposed to be locomotor ataxia, and he thought their history and the treatment employed went far to settle the question of curability of posterior spinal sclerosis.

The first case was that of a patient whom he presented to the Association at its last annual meeting, when he exhibited him as a case of locomotor ataxia cured, and he was happy to say the cure had so far stood the test of time. Dr. Hammond then gave the summary of a case published by Desblat in *L'Union Médical,* November 29, 1883, and also a case which had been treated by Dr. W. A. Hammond about two years ago, and in which all the symptoms of locomotor ataxia disappeared under treatment, and the patient had remained perfectly well up to the present time. Case four was one published by Dr. Friedrich Schultz, of Heidelberg, in which all the symptoms disappeared and the cure remained apparently permanent at the end of two years. The patient remained well up to the time of his death, which occurred suddenly during an attack of acute intoxication. Nine hours after death an autopsy was held. The spinal cord was found to be small and soft, and there was found diffuse degeneration in the posterior columns in the lumbar region, together with degeneration of the outer division of the posterior columns of the dorsal region, and slight degeneration of the column of Goll. Dr. Hammond regarded this as a most instructive case. How it is possible for sclerosis of the posterior column to exist without the manifestation of any symptom of ataxia except a slight eneuresis can only be accounted for in one way. In such a case we are forced to conclude that some other condition of the cord, co-existing with or occurring separately from the sclerosis, is capable of producing the majority, if not all, of the symptoms of ataxia, and that this morbid state under certain conditions is capable of being removed by appropriate treatment. In support of this view Dr. Hammond quoted two cases, reported by Disnos at the Société Médical des Hôpitaux, July 13, 1883. After referring to other cases, and giving a *résumé* of the cases in which cure had apparently been effected, the author of the paper arrived at the following conclusions:

1. That the absence of the patellar tendon reflex in loco motor ataxia is not always caused by sclerosis of the posterior columns.
2. That sclerosis of the posterior columns may exist without being accompanied by the ordinarily prominent symptoms of ataxia.
3. That congestion of the posterior half of the spinal cord may give rise to most, if not all, of the symptoms of locomotor ataxia.
4. That it is impossible during life to make a differential diagnosis between posterior spinal sclerosis and posterior spinal congestion.
5. That posterior spinal congestion is curable.
6. That there is no evidence to show that sclerosis once existing in the spinal cord has ever been removed.
7. That those cases of co-called locomotor ataxia which have been cured are simply cases of spinal congestion, more profound in the posterior half of the cord.

Remarks on Dr. Hammond's paper.

Dr. W. A. HAMMOND.—I did not intend to open the discussion, but as I am obliged to leave within a few minutes I will now say what I have to say. I think that, perhaps, the view that congestion and not sclerosis is at the basis of those cases which have been said to be cured, is probably correct. But upon that point I will not go further into details, and will only bring to mind a single case which has been under my observation lately. It is that of a weaver, an Englishman, who lives in Newark, New Jersey, and who is a very intelligent man. I first saw him one month ago last Sunday, when he was suffering with all of the symptoms of locomotor ataxia. Lightning pains, exceedingly severe at night, cushiony sensations in the soles of the feet, incontinence of urine, lack of co-ordination to such an extent that he was unable to walk a single step with his eyes closed, entire absence of tendon reflex in both legs, double vision occasionally, difference in the size of the pupils, although both were largely contracted, one more so than the other. There was a history of syphilis apparently, although he was not sure upon that point. I prescribed iodide of potassium in large doses. The treatment did not affect him in the slightest beneficially, and at the end of two weeks he was in about the same condition as when I first saw him, although he was taking sixty grains of iodide of potassium three times a day with a thirtieth of a grain of bichloride of mercury. I then changed the treatment entirely, and administered codeine to give him sleep, for lack of sleep seemed to be producing deleterious effects upon him as much as any thing, and I also added one fourth of a grain of nitrate of silver three times a day, without any anticipation of special benefit following the use of the silver. But two weeks afterward to my absolute surprise, all the pains had ceased, the patient could walk perfectly well, the tendon reflexes had returned and were almost exaggerated. I can not account for such results except upon the theory of congestion affecting the posterior part of the spinal cord most markedly, and shifting to the lateral columns. So far as the ataxic symptoms went, they were entirely absent. I do not think that could be regarded as a case of sclerosis, although there were all the symptoms of locomotor ataxia present when I first saw the patient.

Dr. BARTHOLOW, of Philadelphia.—I quite agree with the author of the paper that cases of locomotor ataxia can be cured, but what kind of cases? I was once sharply criticised for reporting a case of locomotor ataxia accompanied by the statement that it could be cured by large doses of iodide of potassium, but it was a case of mercurial ataxia perfectly well developed which got well. I have no doubt that as the metals are so largely introduced into our common modes of living that there are a great many cases of so-called locomotor ataxia which are examples of metallic poisoning. They may be perfectly typical and not distinguishable in any possible way from what should be regarded as genuine cases.

I think, also, we should draw a distinction between two classes of cases undoubtedly syphilitic. Syphilitic gummata of the spinal cord will produce symptoms of locomotor ataxia and iodide of potassium will remove them. But there is a condition of the system, induced by syphilis in its chronic form, favorable to changes in the connective tissue, changes known as sclerosis, in which the results are altogether different, and we should make a distinction with regard to conclusions drawn with reference to the action of remedies between those cases due to gummata and those due to syphilis, which acts secondarily as a cause. In that restricted sense I believe that locomotor ataxia can be cured. Furthermore, I believe that by the use of remedies unquestionable cases of locomotor ataxia can be arrested. There is, however, a great difference between arrest and restoration to the status in quo. I do not believe that any case of locomotor ataxia is curable in the sense that the status in quo is restored, but that the disease is curable in the restricted sense mentioned I have no doubt.

Dr. J. LEONARD CORNING, of New York.—The fact that the ataxic symptoms had been found during life, and autopsy made and nothing further found except hyperæmia, is very conclusive kind of evidence and must be accepted. Moreover, the theory of the possibility of reducing such symptomatic phenomena is perfectly possible on the ground of analogy. We all know the influence exerted upon the heart action by the ebb and flow of the intracranial blood, and the temporary aberrations of the senses produced inside of the cranial vault. Why may not similar conditions prevail in the spinal cord? That the spinal cord is very vascular, and that fluctuation can take place within it, is proven by displacement of the cerebro-spinal fluid, and it seems to me, granting that such a state of things does exist, that hyperæmia is the starting-point, and that if we can arrest the hyperæmia we can arrest further deposit of inflammatory tissue. It is reasonable to say that in the striking case presented by Dr. Hammond the marked improvement in the symptoms was due to the efficacy of the therapeutical measures employed, and in the argument the case greatly strengthens the argument in favor of the hypothesis of congestion.

Dr. G. M. HAMMOND.—I may be pardoned, Mr. President, for making my remarks at this point of the discussion, as I shall be obliged to leave within a few minutes. I do not consider that symptoms resulting from spinal congestion and poisoning from metals, although they are similar to ataxia, are ataxia. Ataxia, strictly speaking, is sclerosis, and what I hold is that locomotor ataxia, true and simple, is never cured. But that these cases which are cured are those in which the symptoms result from some other trouble, whatever that may be, true sclerosis is never cured.

Dr. WEBBER, of Boston.—Last year I reported two or three cases of restoration of the tendon reflex after it had been abolished in patients in whom it had previously been present. In one of these cases I doubt whether the affection of the spinal cord was locomotor ataxia, as the patient had recovered two or three times and then relapsed. I do not suppose it was genuine locomotor ataxia, but he had all the symptoms of the disease.

In another case reported last year I was somewhat in doubt. It was still evidently a case of locomotor ataxia, but the tendon reflex returned. Another case which I reported was one which I saw before the attention of the medical profession was called to the absence of the tendon reflex, and the patient virtually recovered; but afterward, when I examined him, the tendon reflex was present. In that case an autopsy has been made, but the spinal cord has not yet been examined microscopically. Déjerine has lately reported a case in which there was diagnosis of locomotor ataxia, but when the autopsy was made the spinal cord was found to be perfectly healthy; but there was inflammation of the nerves, multiple neuritis. Some of these cases of apparent recovery from locomotor ataxia may have been cases of neuritis instead of lesions of the spinal cord. I should be rather disinclined to accept the view of the possibility of congestion of the spinal cord lasting for weeks and months without producing any organic change, and should be inclined to say it has not been proven that such a condition does exist.

Dr. A. D. ROCKWELL, of New York.—Nevertheless, I believe that the cases of locomotor ataxia which are cured, or greatly benefited, are so few compared with the entire number, that practically it means but very little. In looking over my records I have found that I have had under observation forty-four cases in private practice, and while in the beginning I thought I had cured a number of cases, I am now quite convinced that those cases were not due to true sclerosis. Dr. Webber has also referred to a case in which the diagnosis of locomotor ataxia was made, but where the autopsy showed that the lesions of locomotor ataxia were not present, but that the symptoms were due to other conditions. I have one case in which stretching of the spinal cord was performed with some benefit, and afterward the stretching was resorted to and the patient died, and the autopsy revealed the same condition which Dr. Webber has mentioned, namely, peripheral neuritis. In those cases, which are not locomotor ataxia, I think static electricity does some good.

I reported some years ago a number of cases of locomotor ataxia, or cases simulating spinal sclerosis, and out of the fourteen two recovered, and in one of these the tendon reflex was absent, but it returned after treatment and the patient improved.

Dr. MASSEY, of Philadelphia.—As to the cases of alleged cure of locomotor ataxia, I would like to add one occurring in a gentleman forty-two years of age. He came under my observation three years ago, the case being one of four years' duration. He was an explorer, and had not taken special care of his health. In 1866 he suffered from syphilis slightly, nothing beyond the existence of a hard chancre. Three years ago he began to have visual troubles. There was loss of tendon reflex, which was present before his sickness. He had all the symptoms of locomotor ataxia in the early stage, inability to stand with his eyes closed, sensation of constriction and pressure about the body, lightning-like pains, and decided anæsthesia below the knees, sensation of standing on cushions, and a decidedly ataxic gait. He became quite melancholic. After years of treatment he began to get well, and now three years since I first saw him he is apparently entirely cured. There is no sign, however, of return of the patellar tendon reflex. The treatment consisted in the use of proto-iodide of mercury, with galvanism to the spine and the faradic brush to the extremities. And I may say here that I think the faradic brush is an excellent expedient, it having a physiological basis. The iodide of potassium was administered in large doses at first, but it aggravated his condition and pulled him down so, that it was discontinued.

Dr. C. K. MILLS, of Philadelphia.—I have seen many cases of locomotor ataxia, and also cases of what may be called pseudo-locomotor ataxia. But I have not seen cases of what might be termed the regular type of the disease cured. When I say regular form or type of the disease, I mean that type which we recognize pathologically as posterior spinal sclerosis. I have seen, however, several cases with ataxic symptoms relieved and cured. I believe those cases with ataxic symptoms in which there is a distinct spyhilitic history will be relieved by the energetic use of anti-syphilitic remedies. Some have probably real gummata of the cord, but I believe there is also a form of meningo-myelitis, a low grade of inflammation involving the membranes, and the extreme periphery of the cord itself, in which there are symptoms which simulate those of typical advanced sclerosis.

Again the cases of neuritis will cover a certain number more of those cases of locomotor ataxia reported as having been cured, and some of these cases of neuritis I believe become eventually sclerosis itself.

There are forms of general neuralgia which are cured and supposed to be locomotor ataxia. It is also well known that we have hysterical locomotor ataxia. In some instances the knowledge of having been exposed to syphilis causes the disease to be regarded as syphilitic, which is not syphilitic, but rather hysterical, and these cases are cured in the process of time. True hysterical locomotor ataxia is cured, of course, but in this disease we do not have absence of the patellar tendon reflex.

I should think it quite possible that in Dr. Massey's case there was a large hysterical element, although there was undoubted history of syphilis.

Congestion of the spinal cord may give rise to many of the symptoms of posterior spinal sclerosis, but my own impression is that we cannot really say that we have a limited congestion of a certain region of the cord. We have however congestion of the spinal cord which can be diagnosticated, and it may be that in some cases congestion prevails most extensively in certain regions. Probably we have a condition of meningeal congestion or real meningeal inflammation which gives rise to these symptoms. The point which I wish to impress is this: that the typically recognized form of locomotor ataxia which is pathologically posterior spinal sclerosis is not curable.

Dr. MASSEY.—My case was evidently a typical case, or rather one which presented all the typical symptoms except pain, and including the lessened power of the sexual organs which has since improved. The patient was excessively nervous, but not given to introspection. I do not think it was a case of hysterical locomotor ataxia.

Dr. BANNISTER, of Chicago.—The discussion reminds me of one or two cases. In one there was doubt whether there was any history of syphilis, but I advised a treatment as though it was syphilis, and I reported the case nearly one year afterward as much improved. It then disappeared from observation. The other case was one in which there was a history of illness of only four or five months together, and the patient had every symptom of locomotor ataxia except there was exaggerated patellar tendon reflex. I diagnosticated myelitis involving the posterior roots and probably the lateral columns. I have had several cases in which there were symptoms of locomotor ataxia, with myelitis running a rapid course. I would ask if physicians have not observed amelioration of symptoms from enforced rest.

Dr. W. R. BIRDSALL.—I would like to report the case of a woman who gave a history of characteristic pains of locomotor ataxia for several years, and had the true ataxic walk. No other symptoms of locomotor ataxia were present except the absence of the tendon reflex. She was under treatment by galvanism applied to the spinal cord, which gave relief to the pain, and within eight or nine months all the ataxic symptoms were relieved. Up to this time the characteristic Argyle-Robertson pupil had not been present. One day, in testing the patellar reflexes, I found that she again had the reflexes, although slight, yet it was decided. The ataxic symptoms have not returned. There is beginning atrophy of the optic nerves, the Argyle-Robertson pupil has developed and she is having now areas of pains in the upper extremities. If I had not made repeated examinations myself, and had examinations made by most competent observers, I should have hesitated to pronounce with reference to the absence and return of the patellar tendon reflex. Now here is a case which, if I had written it up at the time, I could have reported as one of locomotor ataxia cured, and indeed the patient was cured for six months, and remains so as far as symptoms of ataxia are concerned. Still I regard it as a case of locomotor ataxia, and one in which there is no history of syphilis. In my own mind it seems to me that in this case we may have some involvement of the lateral columns, a lesion of which often increases the excitability of a part of the reflex arc; and it is possible that in many of these cases the arc is not completely interrupted, but interfered with to such an extent that with the ordinary reflexes we get no response, while even weaker impressions may give rise to reflexes, when other parts of the reflex arc have their excitability increased. It is only upon such a view as this that we can explain the return of tendon reflex in long-continued cases.

In another case, so far as the ataxic symptoms are concorned, the tendon reflex has not returned, but the reflexes in the thigh muscles are quite active—reflexes which are not usually observed. The ordinary patellar reflex is still abolished. since the ataxia has disappeared he has had almost all the visceral symptoms of locomotor ataxia, such as gastric and laryngeal crises, and also the peculiar cutaneous disturbances of sensibility, particularly in the upper extremities, to heat and cold, numbness and pain, etc., for one or two years.

I think many cases have been reported as cured in which there is a long interval of interruption of symptoms, and that a sufficiently long time had not expired to enable the observer to reach the conclusion of the story. On the other hand, there are many functional ataxias which may present or simulate in many respects true sclerosis of the posterior columns; and, further, we may also have these irregular cases where localized lesions exist in some part of the cord which involve the lateral columns as well as the posterior columns, and give rise to a great variety of symptoms simulating true posterior spinal sclerosis.

With regard to the theory of congestion, I will not attempt to pronounce any opinion, except that, as has been mentioned, it seems to me that long-continued congestion can not exist without further changes than those which are merely vascular.

Dr. S. G. WEBBER, of Boston, then read a paper on "Multiple Neuritis."

Neuritis has lately assumed an importance which formerly was not accorded it. Local traumatic and rheumatic neuritis have been observed many times and their symptoms have been well described. The grosser and the more-marked anatomical changes were well known to the profession years ago. The secondary degenerations which rise in the peripheral ends of divided nerves have been known since Waller described them. More recently various trophic changes in the skin have been correctly referred to disease of the nerves.

It is only within a few years, that a more general inflammation of the nerves has been recognized. As has frequently occurred, the disease has been seen, and cases reported at intervals, but attention was not specially attracted to the subject until Joffroy in 1879, Leyden in 1880, and Stewart in 1881, had each described cases. Even then, it was some months before the possibility of a general diffused neuritis was fully recognized, if indeed it is as yet.

Up to this time Dr. Webber had found, in the Boston City Hospital, records of *six* cases without autopsies, and *twelve* cases with autopsies ; one, however, very brief. Dr. Webber gave a summary of each case.

In case 4 the autopsy was made by Dr. Gannett, who diagnosticated "Osteo-sclerosis of the skull, inhalation pneumonia, capillary bronchitis, injection of the kidneys, acute purulent pyelo-nephritis, central fatty infiltration, and cloudy swelling of the liver."

The various nerves removed showed nothing abnormal in their gross appearance. The spinal cord both externally and on section seemed to be healthy. Microscopic examination, chiefly at cervical and lumbar enlargements, showed nothing abnormal.

Sections of the nerves, stained with osmic acid, the brachial, radial, ulnar, sciatics, pneumogastric, recurrent laryngeal, phrenic, showed disease in all the nerves except the phrenic, the pneumogastric, and upper part of the brachial plexus. The lower ends of the sciatic system were most extensively diseased.

The simplest change found was a thinning of the medullary sheath at Ranvier's constrictions, without break in the axis, and without increase of nuclei. The changes in the distal ends of the nerves were very similar, or identical, with what is found in secondary degeneration after section of a nerve. The medullary sheath was divided into fragments of greater or less extent; the axis-cylinder was discontinuous or destroyed, remaining only where the medullary sheath was slightly changed. The fragments of the medullary sheath were finally reduced to very small granular débris, and much of this was absorbed.

In the fibres which were most altered was found an increase of nuclei and apparently small masses of protoplasm, not nuclei, united and in the midst of the granular débris. Cross-sections of the branches of the sciatics showed almost no nerve fibres with axis-cylinders; the sheath was either filled with granular matter or had collapsed.

In 1883 and 1884 fourteen cases of this kind came under his observation in the City Hospital. There was one child, nine years old, in the list. The oldest patient was fifty-one years of age. More than one half were between twenty and thirty years of age. Their stay in the hospital varied from two days (a fatal case) to six months. In most cases no cause could be given for the disease; in five it was attributed to "catching cold," etc. Of these, with other cases, making nineteen in all, nine occurred in females, and ten in males. There were three deaths.

Disturbance of sensation was one of the most constant and prominent symptoms. Within a comparatively short time the pain becomes severe, and is confined to one nerve district, or may extend to several; sometimes the patient can mark the course of the nerve by the pain.

The character of the pain is aching, shooting, or burning. With the pain there is marked hyperæsthesia, and tenderness of the muscles. After the hyperæsthesia has subsided there is diminution of tactile sensibility and other sensations. The special senses have been only rarely found affected.

Motion is early disturbed; at first stiffness, in part due to pain; later paralysis more or less complete, soon followed by contraction of the limbs which sometimes becomes extreme.

The various tendon reflexes were generally absent. The cutaneous reflexes were less disturbed.

The temperature and pulse were rather elevated in the early stages; in the later the temperature dropped nearly, or quite, to the normal, but the pulse continued rather high.

General wasting of tissues and loss of flesh were prominent symptoms in some cases; in others there was œdema of the limbs and even of the face.

Excessive sweating was observed in some of the patients, and the disease began most frequently in the legs, sometimes confined to them, but often extended to the arms.

The disease is to be diagnosticated from anterior poliomyelitis by the pain and hyperæsthesia, the tenderness over the nerve trunks, etc. Progressive muscular atrophy does not have the sensory disturbances; the electrical changes also are not the same. In lead paralysis also the sensory disturbances are usually less marked and the progress is slower. The disease may be mistaken for rheumatic fever, and when there is œdema of the limbs there may be reason for making such a mistake.

Déjerine has recently reported a case which was supposed to be one of locomotor ataxia. The disease in Japan known as Kak-Ke has been described by Scheube, and in every case, twenty in number, he found neuritis.

Recently Dr. J. B. de Gacerda claims to have discovered the bacillus of beri-beri, which can be cultivated, and when injected produces the disease. The nature of the pathological processes in these cases was generally believed to be inflammation of the nerves, but Erb had made two suggestions: (1) that it is possible, and probable, that purely functional disturbances of the trophic centres in the spinal cord produced anatomical disturbances in the periphery; (2) that it is possible that, besides the complete destruction and separation of the trophic centres, yet other pathological processes occur in them which need not always go so far as to show themselves in total degenerative atrophy of the peripheral regions of these centres.

These suggestions were answered at the same meeting, by reasons which seem to show that Erb's hypotheses were less likely to be correct than that which refers the lesion to the peripheral neuritis, and Erb replies that his hypotheses are met only by hypothesis.

In the case Dr. Webber reported, the degeneration in the peripheral ends was simply the degeneration due to the separation of a nerve from its trophic centre.

Considering the changes found around the annular constrictions, it is reasonable to regard the disease a neuritis not dependent upon changes in the spinal cord.

In most cases the patients recovered more or less completely, but recovery was very slow in severe cases; it required much time and patience to straighten the contracted limbs, and strength was regained very gradually. Dr. Webber was not sure that any treatment shortened the attack. Morphine was required to alleviate the pain, and it was given hypodermically. Salicylic acid seemed to cut short the pain in some cases.

Remarks on Dr. Webber's Paper.

Dr. ROCKWELL, of New York.—I would ask if in those cases in which prescriptions were made for the relief of the symptoms the actual cautery was used?

Dr. WEBBER.—The actual cautery was not used in any of the cases in the City Hospital.

Dr. ROCKWELL.—I have had, during the last winter, a case of neuritis in which the reaction of degeneration was present, with the burning sensations upon the limbs especially, and I treated it by internal medication and electricity without very marked benefit. Finally, I touched the sensitive spots with the actual cautery, and I suppose I performed that operation a half dozen times within six weeks, with the result of affording very great temporary relief at least. As to its ultimate effect I cannot say. I do not at present see any special difference in the amount of distortion, but the electricity relieves the pain.

Dr. WEBBER.—I have no doubt that the actual cautery would be of great advantage, but I have found blisters more acceptable to the patients, and they have relieved the pain.

Dr. ROCKWELL.—According to my experience, patients have not objected to the use of the actual cautery, especially after the first application.

Dr. WEBBER.—The trouble with me has been to get the patient to submit to the first application.

Dr. BIRDSALL, of New York.—I think that Dr. Webber's paper is a valuable contribution, and has given us a good deal of material in a new line of observation. What was formerly believed to be myelitis has had more attention drawn to it by the few cases which have been published concerning neuritis.

I have been interested with regard to the possibility of these troubles being due to central lesions, and I am not satisfied that they are not dependent upon central changes. I enunciated that view in the report of a case of lead paralysis, mentioned at the last meeting, in which I found cell changes in the anterior horns of the cord, and ventured to say at that time that I thought it could not be asserted that, because the changes were only found in the peripheral nerves and muscles, the condition was not of central origin. Still, it is a theory which no one can prove at the present time. Dr. Webber's communication is also interesting in the fact that there should have been such a collection of cases, and I think that the relation of the disease to infectious diseases, which has been reported, is interesting, and may suggest further investigation.

Dr. R. W. AMIDON, of New York, presented a left brain which exhibited a softening of the angular gyri and first temporal in "a case of word-deafness and blindness without paralysis." He then gave up his place on the programme to Dr. Roberts Bartholow, of Philadelphia, who read a paper entitled "Note on the Chloride of Gold and Sodium in Some Nervous Affections."

In that curious accumulation of mediæval learning—"The Anatomy of Melancholy"—gold is mentioned as one of the important remedies. From the earliest times gems and precious stones, and to some extent the noble metals, were supposed "to free the mind and mend the manners." Chaucer, who died in the year 1400 A.D., in the prologue to the "Canterbury Tales," describes "a doctour of phisike" who

> "Was but esy of dispence :
> He kept that he wan in the pestilence,
> For gold in phisike is a cordial ;
> Therefore he loved gold in special."

The Arabian and Italian physicians, and more recently Chrestien of Montpellier, recognized the value of gold in certain mental and nervous diseases. Hahnemann also investigated its actions and uses, his "provings" agreeing with the published observations.

From the various sources of information now available, we learn that gold has the property called "alterative" formerly, and now designated by the phrase, "promoting tissue metamorphosis or metabolism"; and it has the power to give stability to nervous matter. The so-called alterant effects, the actions on the nervous system, and the urinogenital properties were the topics treated in the paper.

The preparation preferred for administration is the chloride of gold and sodium—the chloride not being so readily diffusible and manifesting a disposition to clog the kidneys. In small doses it promotes constructive metamorphosis for a time, but in full doses and when administered for a lengthened period increases waste. The tissue yielding most readily being the connective, especially that of pathological formation. Hence its utility in *sclerosis*, whether nervous, hepatic, or renal. Given early and kept up persistently, it has seemed to arrest the progress of posterior spinal sclerosis. In chronic interstitial nephritis it has, in many instances, apparently effected cures.

It has been found very useful in hypochondriasis, especially that form which occurs at the period of degenerative changes in the cerebral vessels. Nervous diseases characterized by spasm, as asthma, laryngismus stridulus, singultus, chorea, etc., have been much benefited in suitable cases.

In sexual debility with hypochondrias it has been found quite effectual in many instances, and in simple inactivity of the sexual organs has proved to be a valuable excitant. In chronic metritis with dysmenorrhœa the persistent use of gold with sodium chloride has appeared to be an effective remedy.

In these groups of maladies are found the types of cases most suitable for the use of this remedy, but further experiences are necessary. Hence these suggestions are made with the view to awaken inquiry, and to secure more extended observations, on which alone can any durable therapeutical rules be founded.

Remarks on Dr. Bartholow's Paper.

Dr. C. L. DANA, of New York.—I am acquainted with one case, somewhat hysterical, in which the patient suffered from mental depression, and was treated with homœopathic doses of gold and always with relief. The patient was treated off and on in this way for several years, and gold was the remedy which relieved her. Whether the effect was mental or therapeutical I do not know.

Dr. BANNISTER, of Chicago.—Several months ago I made a number of experiments in the treatment of melancholia by the use of gold, and the results were not definite. But that does not prove any thing concerning the use of chloride of gold and sodium. The treatment was continued for a number of weeks, and the results were not sufficiently beneficial to warrant me in continuing the use of the remedy.

The Association then adjourned, to meet on Friday, at 2:30 P.M.

Friday, third day, afternoon session.

The Association was called to order at 2:30 P. M. by the President.

The minutes of the last session were read and approved.

Present.—Drs. Ott, Birdsall, Amidon, W. A. Hammond, G. M. Hammond, Walton, Mills, Bartholow, Jacoby, Dana, Spitzka, Rockwell, McNutt, Putnam, Shaw.

The SECRETARY called for the report of the committee, consisting of Dr. C L. Dana and Dr. C. K. Mills, appointed by the President last year to prepare a letter or minute with regard to the death of Dr. George M. Beard.

Dr. MILLS said that he had no report to make, but that the matter had been talked over between himself and Dr. Dana, and the report would probably be made before the close of the meeting.

MISCELLANEOUS BUSINESS.

The Secretary, Dr. AMIDON, introduced the subject-matter of "The William A. Hammond Prize," and asked Dr. Hammond if he would kindly explain to the Association as to whether the prize was still under his control, or whether he had donated it to the Association.

Dr. AMIDON then read the following extract from the proceedings for the year 1882:

"President HAMMOND said he would very gladly renew his offer of the above-named prize for another year. If, at the end of that time, the prize was not awarded, he would donate it to the Association, or to the New York Neurological Society, with the understanding that it should form the basis of a permanent fund for the promotion of orignal research."

Dr. HAMMOND said that he adhered to the statement read by the Secretary, and that, as he understood the question, under that statement he had the alternative of giving the prize to either the American Neurological Association or to the New York Neurological Society. Just what he should do with the prize he was unable at present to say. He thought it probable that he might modify the conditions of the prize, as perhaps the conditions were too rigid. His desire was to obtain an original essay on the "Functions of the Thalamus in Man," and that the conditions were too rigid seemed evident from the fact that no essay had yet been offered which met the requirements.

The Secretary acknowledged the receipt of letters of regret from Drs. Schmidt, of New Orleans; Eskridge, of Philadelphia; Weir Mitchell, of Philadelphia; Clevenger, of Chicago; Jewell, of Chicago; and Seguin, of New York.

The Secretary also acknowledged the receipt of four monographs sent to the Association by Dr. Bernhardt, associate member.

He also announced the reception of a communication from E. C. Seguin, nominating Dr. Auguste Forel, of Zurich, Switzerland, for associate membership.

The Association then elected Dr. Danillo associate member.

Dr. E. C. SPITZKA offered as an amendment to the by-laws: "That all business not of a scientific nature shall be transacted in executive session; an executive session shall be held after each regular session."

Dr. W. A. HAMMOND offered the following amendment to the constitution: "Article VI. of the constitution shall be amended by striking out the words 'Third Wednesday in June,' and inserting 'First Wednesday in May.'"

The Society then proceeded with its scientific work.

Remarks on Dr. Wilder's Resolutions.

The SECRETARY read the following resolutions:

Resolved, That there be appointed by the Chair a Committee of five on Macroscopic Encephalic Nomenclature, with instructions to report at the next meeting a list of such terms as, in their judgment, may properly be recommended for use.

Resolved, That, in the opinion of this Association, the advancement and dissemination of an accurate knowledge of the macroscopic anatomy of the brain will be facilitated by substituting for many of the polynomial terms, technical and vernacular, now in use, technical names which are brief and consist each of a single word.

Resolved, That we recognize the advantages of using such monomial compound terms as auliplexus, diatela, mesocœlia, hemiseptum, præcommissura, medipedunculus, postcornu, and cognate, words proposed by Prof. B. G. Wilder, and see no serious objection thereto.

The SECRETARY then moved that the resolution providing for the appointment of a committee of five to report at the next annual meeting be adopted.

The resolution was adopted, and the President appointed as such committee Drs. Wilder, Spitzka, Gray, McBride, and Birdsall.

Dr. DANA moved that the consideration of the other resolutions be postponed until next year.

Dr. BIRDSALL thought it unnecessary to postpone the consideration of the resolutions, as they could be acted upon then as well as at any other time.

Dr. DANA said the resolutions contained specific recommendations, and it seemed to him that it was unnecessary for the Association to act upon them during the present meeting.

The motion to postpone was lost.

Dr. BARTHOLOW moved that the resolutions be referred to the committee just appointed by the President. Carried.

The PRESIDENT called Dr. Birdsall, Vice-President, to the chair, and then proceeded to read his paper entitled, "The Effect of Injuries of the Spinal Cord upon the Excretion of Carbonic Anhydride."

The effect of injuries of the spinal cord upon the excretion of carbonic acid has been, so far as I know, little, if at all, noted. The action of complete division of the cord upon the pulmonary exhalation has been observed in the course of calorimetric experiments by others. That the gray and white matters of the spinal cord have different functions is axiomatic and it is an important question, in what relation they stand to the excretion of carbonic acid. This subject also has relations to the path of the thermo-inhibitory fibres in the spinal cord. The experiments were made with rabbits and cats by means of Woroschiloff's instrument. The apparatus of d'Arsouval was heated to 100° F., the rectal temperature was taken, and the animal placed in a chamber.

The reason of maintaining the calorimeter at so high a temperature was that I wished to see the effect of the partial section of the cord on the rectal temperature. If the ambient temperature should be much lower than that of the animal, then so much heat would be dissipated through the vaso-motor paresis that no rise of rectal temperature would take place. It is known that after complete section of cord, heat-dissipation being prevented, a rise of temperature will be dependent upon that of the surrounding air whether it is above or below that of the animal. Thus if the spinal cord of a rabbit be cut about the junction of the dorsal and lumbar region the temperature falls, but if the animal is placed in a medium where the temperature approaches that of the body of the animal, the temperature of the animal rises. This rise of temperature is due to the division of the cord and not to the external heat.

Thus if an uninjured animal is placed in a warm chamber, for some hours no rise of the bodily temperature takes place, but when the cord is divided and the animal replaced in the warm chamber then the temperature rises. If the spinal canal is opened, completely exposing the cord, without cutting it, and the animal placed in a warm chamber, the temperature rises only a few tenths of a degree. If on the following day the same cord is divided and the animal placed in the warm chamber, the temperature whilst at first falling, rises several degrees.

Prof. H. C. Wood has made six calorimetric experiments during which the cord was completely divided and the amount of carbonic acid noted. The animals experimented upon were dogs, and the cord was divided low down in the cervical and dorsal regions. In his six experiments the carbonic acid was increased, except in two, and in one of these much blood was lost. He has drawn no conclusions about the carbonic acid. In my seven experiments upon partial division of the spinal cord an increase of carbonic acid was noted in all except two. It made no difference in the majority of them whether the white or gray was alone divided. In relation to temperature and partial section of the cord no rise was seen above that observed after the animal had been an hour in the calorimeter, except in two; in the others it fell below normal. The rise of temperature took place where little beyond the spinal gray matter was divided.

Remarks on Dr. Ott's Paper.

Dr. AMIDON asked Dr. Ott what the bulk of the apparatus employed was.

Dr. OTT said it was about eight feet in length by three in width.

Dr. BARTHOLOW.—I have no doubt of the accuracy of Dr. Ott's experiments, but it seems to me that there is a general source of fallacy in all similar experiments which is not always heeded, and that is the variation of temperature in rabbits, which is due merely to confinement.

Dr. SPITZKA.—I would ask the President whether the number of respirations was noted.

The PRESIDENT.—They were not. I perhaps should have made the description of the mode of performing the experiment more complete. The temperature of the rabbit was taken and it was noted that it was the same, for one hour previous to the time that it was put into the chamber, that it was during the hour it was in the chamber. So that the temperature of the rabbits always remained unchanged.

Dr. W. A. HAMMOND.—Is it not possible that any injury inflicted upon a rabbit which increases the respiration and action of the heart would increase the excretion of carbonic acid?

The PRESIDENT.—That may be true, but I do not know that this part of the subject has been investigated.

Dr. SPITZKA.—How high up was the division made?

The PRESIDENT.—It was made at the junction of the cervical and dorsal regions.

Dr. HAMMOND.—It is well known that in birds the temperature runs up very rapidly after injury, and I think it is so with reference to rabbits.

The PRESIDENT.—As a matter of fact the rabbit was in the same condition all the time, including an hour before the operation, and there was no difference in the temperature during the whole time.

Dr. BARTHOLOW.—Is it not true that in rabbits placed in confinement, the temperature falls, due to compelled rest, so that the excretions from the body would be so far interfered with that no deductions could be drawn with any degree of certainty?

The PRESIDENT.—The animal in this apparatus is not fixed, although he is in a chamber. But he can turn around and move about to a limited extent. But he is always in the same temperature. Besides, he is under observation for an hour before, and in the apparatus for another hour immediately after section, and under the same conditions as nearly as possible.

Dr. G. L. WALTON, of Boston, then read a paper entitled, "A Contribution to the Study of Hysteria as Bearing on the Question of Oöphorectomy." While we have every reason to congratulate ourselves that we have cut loose from the ancient view that all hysteria owed its origin to a disordered uterus, we can not overlook the fact that there exists a certain number of cases in which the nervous symptoms are secondary to organic pelvic disease, as peritonitis, cellulitis, or cystic degeneration of the ovaries. Certainly, when the hysterical symptoms occurring in persons possessing no hereditary nervous susceptibility, date from the accession of pelvic trouble, and disappear with its relief, the etiological connection between the local and the general disturbance can hardly be doubted. It is to that class of cases distinguished from those in which the local symptoms are secondary to hysteria, that the question of oöphorectomy should be narrowed. In certain cases of hysterical hemianæsthesia and hystero-epilepsy it seems extremely possible that the implication of the ovaries in an organic disease offers the starting-point for the irritation producing an hysterical condition, and in such cases the prospect of relief from operation is not to be neglected when milder measures have failed, and when the symptoms are severe enough to make life a burthen on account of pain.

As to the anatomical connection between the ovaries and the hysterical symptoms, there can be little doubt that the cortical cerebral nerve cells are the chief sufferers in hysteria, and all symptoms, mental, motor, and sensory, are most satisfactorily explained by increased and decreased activity of these nerve cells, and final centres of sensation and motion, and presumably the seat of mental processes. In the majority of cases of hysteria the cerebral difficulty is probably a constitutional one, and the variation of the tenderness, which rarely fails, is merely a local hyperæsthesia, and in such cases no more can be expected from the removal of the ovaries than from the removal of the sensitive spot on the skin or scalp. In cases of hysteria secondary to pelvic disease, as peritonitis, cellulitis, or cystic degeneration of the ovary, the local tenderness is of more serious import and significance. If organic changes have taken place involving the real nervous supply of the ovarian region, it is probable, by the transmission to the brain through the sympathetic nerves of the irritation thus produced, that the hysterical symptoms are brought on.

To Dr. Walton the explanation of hysterical symptoms on the theory of the constriction and relaxation of the blood-vessels, was by far more satisfactory than the dynamic theory, at least in the class of hysterical patients under consideration. This theory by no means limits cerebral disturbance to one side of the brain; nor, on the other hand, does it preclude a limitation of the vaso-motor change to a small part of the cortex—as, for example, to that section in particular, the seat of the emotions or sensations. At the same time it well explains the tendency of the symptoms to spread over the entire half of the brain and then to the body.

Theoretical considerations with regard to the pathology of hysteria, however, are secondary to the main question: Is hysteria ever set up by ovarian irritation, and, if so, can it ever be relieved by removal of the offending organ? We may safely answer both of the questions in the affirmative, and here the author of the paper quoted Carsten's conclusions (*American Journal of Obstetrics*, March, 1883), who says that "in general I might conclude that Battey's operation is not only justifiable, but really we might say that it is criminal neglect not to perform it in cases which fail to be benefited by other treatment. It is a last resort after every other treatment has failed, in cases which are caused by ovarian disease, such as hystero-epilepsy, dysmenorrhœa due to fibroid tumors, and which either endanger the life of the patient or make her life a burthen to her."

Dr. Walton thought it probable that persistent hysteria might be added to the class of cases already mentioned, the condition being established that the cerebral disturbance is *secondary* to ovarian irritation.

He then cited a case which illustrated the rôle which hysteria may play as an indication for the operation. The patient was twenty-nine years of age, single, and was operated on by Dr. Barss, of Malden, to whom he was indebted for the opportunity to see the patient, and the particulars of which had already been published in the *Boston Medical and Surgical Journal* for June 5, 1884. Dr. Walton gave a detailed account of the symptoms which were present. The operation was performed January 24, 1884. The Fallopian tubes were not removed. Both ovaries, not materially enlarged, were full of small cysts. At the present time, though still complaining of pain in the lower part of the back, the patient eats, sleeps, and feels better, and Dr. Barss had noted sufficient improvement to warrant him in saying the operation was justifiable. The most marked improvement was with regard to the convulsive attacks, previously so severe and frequent, only two slight attacks having appeared since the operation.

Remarks on Dr. Walton's Paper.

Dr. AMIDON moved that the courtesies of the Association be extended to Drs. Emily Blackwell, E. M. Cushier, and M. Putnam Jacobi, and that they be invited to participate in the discussion of Dr. Walton's paper. Carried.

Dr. SPITZKA, of New York, had given serious attention to this subject in connection with a case in which he had opportunity to make a most thorough examination, and he was very far from being as clear in his own mind as the author of the paper had expressed himself concerning the propriety of resorting to this operation. If the theory of vaso-motor paresis, advocated by the author of the paper, be correct, then he could scarcely understand how a radical removal of the cause should not be followed by immediate relief. The amount of improvement, however, which usually follows, is no more than that which sometimes occurs in such patients spontaneously. Certainly the author of the paper could not advocate the extreme view of the connection of general hysteria with disease of the ovaries, and we still are in a position where we can hardly endorse the moderate and limited view which he advances, for the reason that such symptoms occur independent of disease of the ovaries. On the other hand, even in persons of neurotic disposition, extreme disease of the ovaries and appendages occur without any indications for such an operation. It is all very well to base the recommendation upon the theory concerning the essential nature of hysteria, but the fact is we do not know much of any thing about it, and it is just as possible that some of the manifestations may be due to disturbance of the lower tracts and ganglia, as that they have their origin in disturbance of the cortex itself. So far as the theory was concerned, he was inclined to the dynamic rather than to the vaso-motor. The case which he had the opportunity to examine, and to which he had referred, had taught him an important lesson. It was only after a very careful consideration of the case, sustained by the concurrent opinion of a number of eminent medical counsel, that he finally consented to the recommendation that the operation should be performed, because it certainly seemed that the nervous symptoms depended upon disease of the ovaries. The operation was performed, the disease was found to be much less than was supposed, the patient died five days afterward, and there was no change whatever in the symptoms after the operation.

Dr. Spitzka said he was reminded of one case in which Dr. Israel brought before the clinic in Breslau a patient who had had characteristic hystero-epileptic phenomena, and he said to the gentlemen present: "You see this lady upon whom Battey's operation has been performed, and she has been entirely relieved of her hystero-epileptic symptoms." The patient then left the room, when the professor further stated: "You have seen this patient, and have seen that she has been cured; but I will now say to you that only a superficial incision was made in the parietes of the abdomen, and that the peritoneum was not even touched. A sham operation was performed, and the ovaries are there yet, and the patient is cured."

Dr. C. K. MILLS, of Philadelphia, said his own view of the importance of this operation was that in certainly the vast majority of cases in which it had been performed the trouble might have been relieved by other measures. Thus we all knew, who had had much experience with hysteria and hystero-epilepsy, that conditions as aggravated as those reported in Dr. Walton's case could be removed by measures much less capital than this operation. Then, again, he had had opportunity of seeing, in some cases, failure of the operation to relieve thoroughly the cerebral conditions. A case occurred in Philadelphia not very long ago, or at least came under his observation not long ago, of a woman who suffered from hystero-epilepsy with nymphomania. In her case a prominent female physician removed the clitoris and both ovaries. This patient came to the polyclinic several months after the performance of the operation, and notwithstanding the extent of the operation, she admitted on close questioning that she had many of the same inclinations, and also suffered from nervous symptoms which she had before the operation, although not the same, but in many respects worse.

Dr. Mills thought we should hesitate before recommending an operation of this kind for hysteria. His own view was that hysteria was not only not a disease dependent upon ovarian and uterine conditions generally, but it is seldom specially dependent upon these conditions. At any rate he should hesitate a great while before recommending the operation. In another case in which he was asked to give his opinion, the function of the ovaries had never been established at all, and yet in that instance, as well as in another similar instance, the recommendation had been made that the ovaries be removed. In one case the operation was about to be performed, but for some reason there was hesitation. In similar cases he thought the best thing to be done was to establish the ovarian function in order to relieve the hysterical condition.

Dr. J. J. PUTNAM, of Boston, said he could not share with the speakers' views concerning the theory of hysteria. It did not seem to him to be probable that it arose in vasomotor processes. He thought that the hemianæsthesia of hysteria could not be called unilateral strictly speaking, although he could readily see that there was some reason for giving it that designation. With eye-strain, probably there are cases in which it is one of the causes that act upon a feeble nervous system to produce headaches and other nervous disturbances, and probably in a certain number of cases relieving the eye-strain brings about a transient relief of the symptoms to which it gives rise. So in the same way with reference to disturbance of the nervous system in connection with ovarian irritation. If we get the disturbing influence of an important function, and if the organs which give rise to the function can be removed, and if it can be done with the patient's understanding and full knowledge of the gravity of the operation, in a certain number of cases relief may be afforded, and it must necessarily remain one of the means to be tried, although to be resorted to after all other measures have failed.

Dr. A. D. ROCKWELL, of New York.—This paper seems to offer an opportunity to make a suggestion arising from a long experience at the Woman's Hospital of this city.

There is no doubt that the operation is an important one, and one which, as a rule, is performed as it should be performed, and is performed when it should be performed, in that institution. Still, he was impressed with the fact that there was quite a minority of cases where various symptoms which were attributed to ovarian disease really were not dependent upon that disease. He had quite complete records of all the cases which he had treated in that institution, where many cases of ovarian disease had been sent to him for relief. And he had often found that hysterical symptoms, neuralgic symptoms, and symptoms of disorder of sensation and function were relieved entirely, while the ovarian disease remained unchanged. He knew of one case where it had been recommended by an eminent gynecologist that the ovary be removed, but in which the patient was cured without such operative procedure.

Dr. M. PUTNAM-JACOBI had in mind three or four cases which might be mentioned in connection with that forming the basis of Dr. Walton's paper. One was that of a girl twenty years of age, who when she first saw her was said to be suffering from chloro-anæmia. She had never suffered from dysmenorrhœa, although menstruation had been scanty, and she had a certain amount of pain in the ovarian region associated with no objective local symptoms whatever. Some months afterward she heard that the patient had had a transitory attack of paraplegia, coming on suddenly, lasting twenty-four hours, and then passing away. Two months later a similar attack came on, and the patient suddenly became unable to walk, and this attack persisted, and Dr. Jacobi saw the patient after she had been in this condition a number of months. She was not entirely paralyzed, but was unable to walk steadily. The patient was seen by a number of physicians, and among others by Dr. Thomas, who regarded the paralysis as hysterical and not reflex. Three months afterward Dr. Mundé performed oöphorectomy, and, as Dr. Jacobi understood, the patient was able to walk within six weeks after the operation.

Another case which had fallen under her observation was one in which Dr. Cushier performed the operation, and was one in which the local objective signs of ovarian disease were marked, while the hysterical symptoms were very much diminished. In this case both ovaries were prolapsed, sensitive to the touch, and the patient had suffered for fifteen years from severe dysmenorrhœa, and premenstrual pain characteristic of ovarian disease. Pain strictly limited to menstruation can not be considered as dependent upon disease of the ovaries, but only when pain precedes menstruation by one or two weeks can it be referred to the ovaries. In this case the ovaries were in a condition of cystic degeneration ; one half of one ovary was transformed into a large cyst.

It seemed to Dr. Jacobi that whenever the ovaries were large and prolapsed, and continued in this condition for one or two years, and the case resisted various methods of treatment employed for reducing the size of the organs and keeping them in position, we might infer that there is cystic degeneration, and from the time that cystic degeneration exists, the ovaries will have to be removed probably, unless, as occurs in a certain number of cases, when a large cyst forms and the organ becomes converted into an indurated tumor, from which the patient suffers but little pain. She had one such case in her record books. As soon as there is proof that the ovaries are cystic, it is simply a question of giving a reasonable amount of time for other remedies to act, and then the question of removing the organ should be considered.

On the other hand, where there are no objective signs of disease of the ovaries, simply pain in the ovarian region, and still some hysterical symptoms, then it seemed to her that we occupied an entirely questionable ground as to whether removal of the ovaries would cure or benefit, or not. Where the ovaries had been removed for tumor of the uterus it certainly had not had any effect at all.

It seemed to her that this question, always a question with regard to hysteria, whether we have to do with symptoms projected from the cortex of the brain, or whether we have to do with local irritation which has its cause in disturbance of the cortex of the brain, was an important one, and that an immense amount of injustice is done by claiming that pain is imaginary because the patient's attention is distracted from it. If we can find a most singular peripheral sensation which heralds the coming of an epileptic fit, which can be considered as pointing in which region of the brain the epileptic symptom begins, we may probably infer the same thing in these cases of hysteria, in which the source of the disturbance is central and not from the first peripheric.

Dr. WALTON, in closing the discussion, would like to say that there could hardly be any one more conservative than himself upon the question of the operation, except the gentleman who held that it should not be performed at all.

With regard to what Dr. Mills had said, he would simply read a sentence of his paper in which he said : " We can not overlook the fact that there exists a certain number of cases in which nervous symptoms are secondary to organic pelvic disease." It was only in those cases in which he advised the operation. Perhaps later we may add cases in which only functional disturbances are at the bottom of the trouble, although he was extremely conservative with regard to attacking such cases.

The question had been asked : If hysterical symptoms came from the ovaries, why does not removal of the ovaries cause immediate relief. Dr. Walton thought that we have many cases in which secondary disturbance is not removed at once by the removal of the apparent cause,—sometimes it does not disappear at all after the removal of the original cause of irritation, especially when we do not know whether we have done sufficient or not. We may extract a tooth for the relief of a neuralgic pain upon which it is supposed to depend, but the neuralgia may continue, not because the tooth did not produce it, but because the operation had not been properly performed, as, for example, a fragment of the tooth may have been left, or a condition of the jaw remain which was not removed. He thought that one reason why some of the cases had not been cured that might have been cured, was because the Fallopian tubes had been left. Menstruation rarely ceases unless the Fallopian tubes have been removed, but in that respect his case was an exception. The reason why, in his own case, the patient had not been entirely relieved Dr. Barss had attributed to the fact that the tubes were not removed.

It had also been objected that hysterical symptoms sometimes occur independent of all disease of the ovaries. That is a question to be decided, and that fact sometimes makes the choice of cases more difficult, but it does not show that the operation is not justifiable.

It is said that these patients sometimes get well suddenly, and sometimes they are cured by a false operation. In those cases he held that the diagnosis was false, for it was considered that such symptoms were dependent upon diseased ovaries. If the case has been diagnosed hystero-epilepsy secondary to ovarian trouble, and a false incision has been made, and sometimes a recovery takes place, the symptoms were not due to actual organic lesion, but such cases were cases of hysteria cured by shock to the nervous system.

It has been said that many cases which have been operated upon could have been relieved without operation. With regard to that I have already stated we should try to relieve the patient without operation, and resort to operation only when other means have failed. We should hesitate long before operating for hysteria alone. Fortunately, we are rarely called upon to operate for hysteria alone, but usually it is the pain which makes the patient's life a burden, and for the relief of which the operation is demanded.

It has also been said that hysteria is seldom dependent upon the uterus, but is generally constitutional. That is the ground which I hold, and, therefore, think there are but few cases in comparison upon which we should operate. It has also been said that hysteria occurs and has been falsely attributed to disease of the ovaries. That is true, but it does not militate against the operation. It simply proves that a mistake in diagnosis has been made, and it does not affect the question whether the operation is justifiable or not.

Dr. JAMES J. PUTNAM, of Boston, then read a paper entitled: " Typical Hysterical Symptoms in Man Due to Injury, and their Medico-Legal significance."

The following case was offered, not on account of its intrinsic interest, but because it was a marked instance of what he believed to be an important, and, perhaps, not uncommon type of disease. The medical expert was rarely called to the witness-stand under more annoying circumstances than when required to testify as to the condition of a patient who claims to be suffering from the depressing results of so-called "concussion of the spine," the result, perhaps, of a slight railroad jar, yet whose symptoms reveal themselves by no outward sign that may not be ascribed to simulation. Under these circumstances any thing of the nature of objective signs of disease is a godsend.

Some of the symptoms of typical hysteria, though not strictly objective, possess almost the same degree of significance on account of the fact that, taken together, they form a picture familiar to the expert, but such that no layman, in the present state of medical knowledge, will possibly imagine.

Dr. Putnam then gave the history of a case which formed the basis of his paper—an instance of railway accident. The patient was jolted forward, then backward, and finally slid down between the seats, striking and scraping his back with some force against the edge of his iron seat, the cushion of which had been displaced.

He gave an account of the symptoms of the case in detail, and finally summarized with the statement that the patient became at first completely paraplegic, then, after some months, hemiplegic, the motions of the right leg remaining absolutely abolished—even those at the hip-joint,—and it was also soon discovered that the loss of sensation had shown the same tendency and concentrated itself upon one-half of the body, and the case assumed in this respect the appearance of a typical hemianæsthesia.

The special senses were all involved in this impairment, and the hearing was diminished for bone-conduction as well as for air-conduction. The emotional irritability and lack of control, although less marked than at first, had also remained to some degree, although hardly enough to justify a diagnosis if the patient were seen now for the first time.

This was the third case of more or less complete hemianæsthesia in male patients that Dr. Putnam had seen, within a comparatively short time, as the result of concussion, and similar conditions have, of course, frequently been noticed with the other sex, as a result of shocks of various kinds, and it was also well known that a general hysterical state, as well as a variety of marked hysterical symptoms, might make their appearance under like circumstances in men.

A number of cases of this kind have been reported by Page ("Injuries to the Spine"), to whom chiefly belongs the credit of pointing out that Erichsen and his followers are on the wrong tack in seeking to group the symptoms of railway concussion among the signs of spinal disease, instead of referring them to the functional cerebral disorders, where the majority of them unquestionably belong.

The only original observation which his paper sought to establish was that, even when all the prominent symptoms of hysteria, if any were present, have faded away, disorders of sensibility, mainly unilateral, may remain and serve as a welcome guide. It was past belief that a man not previously under the training of a professional expert, should be shrewd enough to assert a set of symptoms which neither court nor jury could comprehend or sympathize with.

Another point of interest was brought out in the course of the trial. In the early period of his sickness the patient had shown an insensibilty of the skin of the legs, which quite abruptly ceased at a line passing across the thigh a short distance above the knee, and then running obliquely upward and backward. The question being raised whether this could have been done by design, Dr. Putnam and Dr. Webber made some experiments with regard to this point, and satisfied themselves that to observe such a line with any thing like accuracy was almost, if not entirely, impossible.

Dr. Putnam believed that it was due to the interests of justice and the credit of the profession if the courts should be led to understand clearly that there is a hysteria which deserves something better than to be mated with a smile and a cold shoulder; that it is an affection which has its own pathology and its own symptoms, and is not a mere mirror of real disease. In the trial upon the case above described this policy was followed and with good effect.

Remarks upon Dr. Putnam's Paper.

Dr. C. L. DANA, of New York.—I have been interested in this paper, because not long ago I was called to see a case belonging to this class. Indeed, I have seen two cases, in one of which I was asked to advise for the patient, and in the other I was asked to advise for the railroad. To begin with Dr. Putnam's last point, I think there is an element in that question which he has not referred to. Hysteria is no doubt a serious disease, and one which the conscientious medical expert should not belittle; but the question arises whether the hysteria is due to the accident, or whether it is developed by the expectation of reward and a constant peculiar mental anxiety concerning some enormous sum of money. The case, which I have had occasion to study rather closely lately, was that of a delicate woman who was on a train during an accident, who was thrown forward from the seat of the car upon the floor. No one else was thrown from their seat, and the accident was not serious. There was no injury except slight contusion upon one leg. After two or three weeks she began to have hysterical attacks, and during this time her husband had put in a claim for thirty thousand dollars damages, which, if received, would make the family wealthy. She developed symptoms of what was called by her physican spinal anæmia, and this went on increasing in severity, and finally she developed bed-ridden hysteria. My opinion was that that woman suffered from hysteria, perhaps starting from the accident, but the expectation of a reward made it worse.

The subject of spinal injuries produced by accidents upon railways is in such a state of confusion, that it is almost impossible to get a lawyer to understand what the position of best experience is. I think it would be well if we could get some authoritative statement, or at least some authoritative statement would be desirable, and some judicial statement would also be highly serviceable concerning

Page's and Erichsen's cases, which are so frequently cited in the courts.

Railroad injuries should be divided into two classes: First, those in which there is a serious injury to the spine of one kind or other, such as contusions, hemorrhages, fractures, etc., and from which the patient suffers immediately, and .continues to suffer for a good while and dies. The other class of cases is divided into two kinds: one in which the symptoms gradually, slowly develop, and the patients finally present a picture of chronic meningo-myelitis of the spinal cord; and another class in which is included those which Erichsen regards as spinal concussion, but which should be really regarded as hysteria and hypochondriasis.

I have been unable to find hemianæsthesia in man, as has been referred to by Dr. Putnam.

I think that the electrical resistance may be of some advantage. In the examination of my case the electrical reactions were pretty nearly normal. There was not much objective disturbance. By testing with galvanism I found that the electrical resistance was greater upon the affected side than upon the other side, although there was no difference in sensibility. Some French observer, whose name I do not recall, has shown that electrical resistance is diminished upon the affected side in hysterical paralysis, and it was noticing this statement that prompted me to make the observation, and I demonstrated a marked difference in the electrical resistance between the two limbs, and I think it was pretty well shown.

Dr. PUTNAM, of Boston.—I am very glad that Dr. Dana has studied this subject, and recognized the fact that it is a very important one. All the points which he has made are extremely proper, but I think experts should come to some conclusion, and the entire subject should be reopened from the point of view from which it has been studied. With regard to the slowness of injury, it does not seem to be an element of much consequence, for the patient may be seriously affected, and that before any trial or claim for compensation has entered the mind, and yet apparently be only very slightly injured. In another case where the accident was not severe, similar symptoms were developed, although no claim whatever for legal damages was made.

The length of time which may elapse after the occurrence of the accident before symptoms develop, I do not think is of much importance, because the effects are sometimes exceedingly delayed. The sensibility, I think, should be tested in all respects, for the hemianæsthesia may be partial. In the first case which I have mentioned, the man was perfectly well, but I found to my surprise that he was able to bear an amount of pain in parts removed from the injury, which rendered it highly probable that he was assuming it.

The question of atrophy is not of much account as, in the last case particularly, and also in one in which I have subsequently made observations, there was a certain amount of flattening of the muscles. A little coldness of the skin-which regularly accompanies such injuries, although it is of no consequence of itself, seems to prove that parts in a state of paralysis must have been active for a long time, and that would be a presumptive basis of argument for simulation, because the patient who simulates would scarcely think of that part constantly, and would be likely to move it somewhat, but very little movement would be necessary to prevent atrophy. With regard to electrical resistance, I do not think that could be of much avail, because the variation depends so much upon the condition of the skin that it could not be made of very much account.

Dr. SARAH J. MCNUTT then read a paper entitled: "Provisional Report of a Case of Double Infantile Spastic Hemiplegia," and presented a brain which showed the lesions.

The patient was a girl of two and a half years—never had walked, never had talked ; all of the limbs were smaller than normal, especially upon the left side. They presented the usual contractures of the spastic state. The patient had stridulous respiration with dysphagia and nasal regurgitation of food. The difficult respiration and deglutition with the paralyses of the extremities dated from birth. The child presented by the feet, the labor was delayed and tedious, and instruments were finally used. For nine days after birth the child had constantly recurring convulsions. It died of pneumonia.

Upon autopsy atrophy of the ascending frontal convolution, of the ascending parietal convolution, of the paracentral lobule, and of the posterior part of the third frontal convolutions, were found upon both sides of the brain, the condition being most marked upon the right side. A very thin band in the callosum connected the atrophied cortex, and secondary degeneration was present in the pyramidal tracts.

The author cited Henoch, Ross, and Hutchinson's cases. She referred to Benedikt's theory of causation, which regards the spastic hemiplegia of infancy as a systematic affection or a poliomyelitis of .the motor cortex, similar in character to the poliomyelitis of the anterior horns in the cord, and she cited a living case of double hemiplegia following measles, in whom she supposed the lesion to have been thus produced. She also referred to the frequency of abnormal presentations and delivery in cases which have infantile hemipligia without an acute disease, and supposes traumatism at birth to have been in these cases the primary cause; in her own case considering the convulsions to have indicated an extensive meningeal clot.

The author hopes to locate the time of the lesion more definitely upon the completion of the microscopic investigation which is being conducted by Dr. William H. Welch.

The Association then adjourned to meet at 8:30 P.M.

Third day, evening session.

The Association was called to order by Dr. W. R. BIRDSALL, of New York, Vice-President.

Present—Drs. Birdsall, Weber, Amidon, Dana, Gibney, Spitzka, Massey, Rockwell, Walton, McNutt, Jacoby, Putnam.

The minutes of the last meeting were read and approved.

The Council reported favorably on the nomination of Auguste Forel for associate member.

The report was accepted, and the Secretary was instructed to cast the ballot for Dr. Forel.

Dr. DANA from the committee to report resolutions on the death of Dr. Beard reported progress.

Dr. BIRDSALL called Dr. Weber to the chair, and then read a paper on "Ophthalmoplegia Externa Progressiva."

He reported two cases, one from the hospital service of Prof. Roosa, the other from the practice of Dr. Wilson, of Bridgeport, Conn., of slowly progressive paresis of all the external muscles of both eyes (the levator palpebræ, the recti, and obliqui), producing partial ptosis and nearly complete immobility of the eyeballs, with complete preservation of the functions of the internal ocular muscles (iris and ciliary muscles); accomodation being normal, and reaction of the iris to light and accommodative movements also normal; no perceptible lesion of the fundus; vision normal in one case, and defective from irregular astigmatism in the other. No evidence of disease in any other cranial nerve or in any part of the body. No headache and no sign or suspicion of syphilis. The patients were both males, aged respectively seventeen and twenty-nine. Slow improvement occurred: in one case under large doses of potassium iodide; in the other under the same drug, and faradization of the eyes. Both are still under treatment.

Dr. Birdsall held that the lesion could not be due to intraorbital disease, nor to an intracranial lesion involving primarily the trunks of the nerves implicated, on account of the escape of those branches of the third nerve, which supply the iris and ciliary muscles; that it must therefore be an affection of the nuclei of origin of the sixth, fourth and eighth parts of the third nerve, which supply the external occular muscles, these parts representing an associated system concerned in movements of the eyeballs, and in lifting the upper lids, somewhat distinct from the iris and ciliary muscles which are more intimately connected with the function of the optic nerve, in the regulation of light, and are probably supplied as far as the sphincter iridis and the ciliary muscles are concerned, from nuclei in close proximity to but distinct from the nuclei of origin of the remaining third nerve fibres, which supply the external ocular muscles. A gross lesion or a focal softening, as a neoplasm, meningitis or an arteritis, could barely affect so widely separated nuclei as the third, fourth, and sixth, and not at the same time affect the ciliary and pupillary centres, those of the fifth and the seventh nerves, or other neural tracts. Degeneration within this system of associated muscles, nerve tracts and centres, similar to the degeneration of progressive muscular, atrophy and of labio-glosso-pharyngeal paralysis, appeared to be the most consistent theory of the pathology of these cases.

Dr. Birdsall stated that Hutchinson, in 1869, had advanced this view, based on an autopsy in which the lesion was found by Dr. Gowers, to account for this class of cases, which Von Graefe first called attention to. In most of Hutchinson's cases, however, the iris and ciliary muscles were affected, and either the conditions or indications of disease in other parts of the nervous system were found in all of his seventeen cases. Reference was made to other allied cases, by other authors; to certain relations between tabes dorsalis and progressive muscular atrophy; also to syphilitic occular affections.

Remarks on Dr. Birdsall's Paper.

Dr. E. C. SPITZKA, of New York.—In such a rare affection the discussion must necessarily be limited to anatomical and theoretical considerations. There is one difficulty in the way of the explanation of the lesion from analogy offered by bulbar paralysis. The nerve nuclei in bulbar paralysis are not remote from each other, but there is a great distance between the nuclei of the fourth and the sixth pairs. It is of course possible that we have to deal with a particular system, and that a morbid influence could be transmitted even through this great distance by a system of fibres which unite these nuclei.

There is nothing remarkable to my mind in that the cilary and ocular muscles supplied by the third pair should escape, for the experiments of Hensen and F—— show that of the many sub-nuclei into which the third nerve can be anatomically divided, those most anterior are the ones in which these mechanisms have their origin, while the most posterior nuclei are devoted to muscles affected in Dr. Birdsall's case. So there is topographically a reason why, while all the other muscles suffer, the internal ocular muscle should have escaped.

Dr. BIRDSALL.—The idea expressed has already been advanced by Hutchinson, and I regarded it as Hutchinson did, as a systematic affection, and that these nuclei are affected functionally. The reason why I felt obliged to consider it a pathological process involving these nuclei and associated tracts was, that a greater difficulty came in the moment we attempted to account for it by gross lesion. Of course these cases are exceedingly rare, and we can not judge very much as we have no autopsies of exactly similar cases. Furthermore, it is possible that these cases which I have presented, if allowed to go on by themselves, may develop new symptoms, although the progress of one case is now something over two years.

Dr. BIRDSALL presented Dr. Seguin's statistics with special reference to the efficiency of syphilis in the etiology of tabes dorsalis. They consisted of seventy-two cases, in twenty-two of which there was history of chancre, 30 per cent.; chancre with secondary symptoms in sixteen cases, 22.2 per cent.; a total of 52.2 per cent. There was no history of chancre or secondary symptoms in fourteen cases, 19.4 per cent.; and no mention was made with reference to syphilis in twenty cases, 28 per cent.; total, 47.4 per cent.

There were, then, in seventy-two cases 52 per cent. in which syphilis occurred, and 48 per cent. in which either syphilis was not mentioned, or there was no history of either chancre or secondary symptoms.

Dr. Birdsall continued with his own statistics which he presented at the last meeting of the Association, when he reported that he had collected 525 cases of locomotor ataxia, of which 225 gave a history of syphilis, or 43 per cent.; of his own cases contributed to that number there were forty-two, of which four gave a history of syphilis, or 9.5 per cent.

Dr. Seguin's statistics were based upon cases in private practice, while his own were based upon cases seen in hospital and college clinic practice.

Dr. AMIDON asked the privilege, which was granted, to submit the following statistics which he had received from Dr. Webber, of Boston.

About half the hospital patients were under Dr. Edes' care. The whole number of patients with locomotor ataxia, 62; of these 7 were women. In 37 cases it was distinctly stated whether there had been syphilis: 20 had had syphilis, 54%; 17 had not had syphilis, 46%. Of the 62, 33 were hospital, 29 were private, patients. Of 37 cases where record was made in regard to syphilis, 19 were hospital patients, 18 were private. Of the 19 hospital patients, 13 had had syphilis, 68%, 6 had not had syphilis, 31%. Of the 18 private, 7 had had syphils, 38%, 11 had not had syphilis, 61%.

In only seven cases, 3 hospital, 4 private, had there been secondary symptoms, at least so far as the patients knew, and in each case the secondary symptoms appeared before the ataxia.

In one case the patient said he had syphilis only two years before the examination, and the ataxic symptoms probably were not so recent, but he acknowledged to having had gonorrhœa ten years previously.

In one case syphilis preceded the ataxia by three years, in one four years, in the others from ten to nineteen years.

In six there was excess of venery, one patient indulging almost daily for fifteen years, another for twelve years, one daily or twice a day for a year preceding the ataxia.

Fifteen of the sixty-two patients are stated to have been exposed to cold or wet, or to both. Several had met with some strain or other injury to the back.

One patient finally died from general paralysis.

Dr. SPITZKA also asked permission to hand in his statistics, of which there were two sets, including a total number of sixty-one patients in both dispensary and private practice. One set included the records of twenty-three or twenty-four private patients which had been under his observation since 1881. The proportion of well established syphilitic cases in the whole number was over eighty per cent. He had one case of well-established cure by antisyphilitic treatment, all the symptoms completely disappearing, and the patient is now in every respect a well man, the only relic of the disease being asymmetry of the patellar jerk.

Dr. A. D. ROCKWELL had the records of forty-four cases seen in private practice. In seventeen of these the patients had a distinct syphilitic history, or about forty per cent. Six cases occurred in women, and of these two were syphilitic. He should say that of these forty-four cases more than seventeen were syphilitic, because in several of them syphilis was denied at first, and was only found out afterward by very close investigation. His opinion was that syphilis is by far the most important etiological factor, and probably exists in the vast majority of cases of locomotor ataxia.

Dr. PUTNAM, of Boston, said he had sent Dr. Seguin the records of thirty-four cases, in forty-nine per cent. of which syphilis was present. In all the cases in which he had tried antisyphilitic treatment it had been unsuccessful.

Dr. SPITZKA said he did not mean to be understood that the changes in the spinal cord in his case had been removed, but there was no progress in the disease. He would further state that he had succeeded in producing the patellar jerk in two cases which afterward disappeared, and the disease took its regular course, terminating fatally.

Dr. ROCKWELL suggested the possibility of the existence of a peripheral lesion in the case which Dr. Spitzka reported as having been cured.

Dr. SPITZKA could not admit the existence of a peripheral lesion, because it was not conceivable how a peripheral reflex should be associated with the Argyll-Robertson pupil, which he considered as one of the most reliable differential signs of the disease. Of the total sixty-one cases there were two which occurred in women, neither of whom were syphilitic.

Dr. PUTNAM remarked that in his cases there were two women, neither of whom were syphilitic.

Dr. L. WEBER, of New York, quoted his statistics which he had given in a paper read before the New York Academy of Medicine, and published in the *Medical Record* for April

5, 1884. Of the 134 cases of syphilis of which he had kept record during the last twenty years, 118 occurred in men, and sixteen in women. None of them had been less than four years under observation, and most of them from ten to twenty years. Eighteen of the patients had symptoms of specific lesion of the central nervous system, 13.5 per cent.; eight of them of the brain alone, 6 per cent.; five of the brain and spinal cord together, 3.7 per cent.; five of the cord only, 3.7 per cent. Two of the eight cases died of cerebral syphilis, the others were still alive, but not cured. Of tabes he had the record of seventeen cases, and of these syphilis was certainly the principal predisposing cause in three—that is, 18 per cent., and in two more of them it had probably been an important etiological factor.

As to the anti-syphilitic treatment where there had been a quick development of tabetic symptoms, perhaps mostly of the lepto-meninges, an energetic anti-syphilitic treatment would indeed sometimes produce such wonderful results as had been mentioned by Dr. Spitzka, but in the slow cases of tabes occurring in persons from thirty to forty years of age with a distinct syphilitic history he had yet to see the first case where there had been any improvement whatever, so far as the tabetic symptoms were concerned, by the use of anti-syphilitic treatment.

Dr. SPITZKA would like to remind Dr. Weber of a case which he saw with his partner during his (Dr. W.'s) absence, and in which he (Dr. S.) gave an unfavorable prognosis. There was unquestionably a central disease of diffuse character. He had received a report from Dr. Weber subsequently that most wonderful improvement had occurred. He thought there was a great deal, however, in what Dr. Weber said in regard to rapidly and slowly developed cases with reference to the ultimate results to be expected from treatment. In his case the patient presented the phenomena of locomotor ataxia for at least two and a half years, and he did not believe that the case was one in which the lepto-meninges were primarily affected. Indeed the entire chapter of syphilitic meningeal affections is really in the dark at present. Dr. Spitzka had made observations in a somewhat analogous disease—namely, general paresis of the insane,—and he had at present two cases under treatment. In one, pure symptoms marked its development, and that patient had improved, and probably would be able to attend to his business again. In another case, with slow progressive symptoms, the treatment had been in vain. In this respect his experience in general paresis was like that which he had obtained in the majority of cases of locomotor ataxia, and it was that these cases would go on in spite of the best or worst treatment. So far as his case of locomotor ataxia was concerned, he regarded it as one of the very few unquestionable cures.

Dr. C. L. DANA, of New York, said that Dr. Weber, by his statistics, had suggested another way of looking at the question. Some years ago he made a study of statistics with regard to the number of cases of nervous disease among sailors, and especially with reference to the relation between nervous diseases and syphilis. As he remembered, the proportion of cases of locomotor ataxia in syphilitic patients was as small as one in a thousand. He made his statistics upon the records of the U. S. Marine Hospital Service. Of course, statistics of that kind would be open to much criticism, and the statement might not be worth much, but he was quite sure that not more than one in a thousand of sailors who were syphilitic had ataxia.

Dr. C. L. DANA, of New York, then read a paper entitled, "*Folie du Doute* and Mysophobia."

He referred to the history of *folie du doute*, or the "Doubting Madness," which had been chiefly studied by the French writers. Griesinger and Berger had also described a psychopathic symptom which belonged to the same category, and had called it "Grübelsucht" or metaphysical mania. In folie du doute the patient is harassed with a constant desire to question, speculate, and refine over details. His mind is never settled, but is in a condition of "pruritus." It is a psychopathic symptom which indicates that the patient is a monomaniac and truly insane or only hypocondriacal or profoundly neurasthenic and hysterical. It is the form in which primäre Verrucktheit sometimes develops. Mysophophia or fear of contamination is a symptom indicating probably a similar condition.

Folie du doute had been divided by Ball into several varieties, such as the suspicious, the calculating, the timid, and, finally, *tactile;* and mysophobia had been classed as the tactile form of folie du doute. This classification was open to criticism, but Dr. Dana was inclined to think it correct. He related two cases: one a typical one of folie du doute, as described by Falret, Ball, and others; the other one of mysophobia as first described by Hammond.

The first patient was a married man of thirty of a phthisical but not neurotic family history, who had indulged in much sexual excess. He was tormented and worried continually as to the ordinary details of his daily life. He had to wash, shave, eat, drink, exercise, work, etc. in a particular way or he was wretched.

The other was a single woman of twenty-eight, of healthy family history but of hysterical temperament, who for six months had suffered from fear of dirt and contamination, showing the typical symptoms described in cases reported by Hammond, Shaw, Seguin, and Russell.

Dr. DANA suggested as points for discussion: 1. The relation of mysophobia to folie du doute and Grübelsucht; 2. The question of the proper interpretation of morbid fears in general.

Remarks on Dr. Dana's Paper.

Dr. SPITZKA had seen within fourteen months eighteen cases in which were found original monomania and imbecility, and misplaced mental powers for many years. He had also been struck by the fact that heredity plays a slight rôle in this class of cases. He could remember only a single male patient, who was at one time healthy and a great masturbator, and the results of treatment had been gratifying, and one of three cases which he had seen improve. In one of these cases the patient had been advised to study medicine by three of his colleagues of this city. The result was, as might have been expected, that he broke down completely, and has more new material. In one of these cases there was fear of glass. This patient insisted upon having every particle of food strained through a cloth, and finally came to eat with wooden spoons and other wooden utensils, and her cure dated from the moment when one morning she was about to leave my office I asked her what she had that veil on for, and quickly removed it, and obliged her to go home without it. In the great majority of cases home treatment is almost impossible. Isolation in small country homes is the best treatment possible without attempting to argue or to inflict self-discipline. He thought there was scarcely any one who had not at some period in his life been the subject of imperative conception. He remembered distinctly at one period in his own life when for two days after he had recovered from the effects of morphine necessary to relieve pain incident to a surgical operation that he had an imperative conception in a dream, and the only way in which he could get rid of it was to obtain sleep.

Dr. WEBER, of New York, said he did not see why mysophobia should be raised to the dignity of hypochondriasis. He considered it simply as a symptom of hypochondriasis in the male, and hysteria in the female; in the male with a history of masturbation or sexual excess frequently, and in the female usually associated with some uterine disease at the bottom of hysteria. He could not look upon mysophobia as entitled to the dignity of a special form of hypochondriasis.

Dr. DANA thought that if Dr. Weber had seen one typical case of mysophobia he would admit that it was a distinctly marked trouble, clinically speaking at least, and not to be confounded with ordinary manifestations of hypochondriasis or hysteria. He did not understand that any author makes of mysophobia a separate disease of itself, only mysophobia is a name for a psychopathic symptom indicating that the person is the subject of a particular form of monomania or insanity. The question whether or not it should be called insanity, it seemed to him, was largely a verbal one. He had a case under observation in which the woman worried constantly where things should go, and had been regarded as eccentric, but she could not be called insane; and other forms were a little worse, and so they increased, and we can hardly say where the patient begins to have abortive monomania,—where you can draw the line and call it insanity.

Dr. PUTNAM, of Boston, was very glad to hear Dr. Dana say that it was not always necessary to remove these patients from home, because if they can be treated without that he should be pleased to hear Dr. Dana give a little more in detail his moral and medicinal treatment, for he had not been able to make the least impression upon any of the patients which he had had. If there were any indications for moral treatment which could be set forth, he thought it would be extremely instructive.

Dr. DANA said he did not think he could give any idea of the measures he took in this particular case. He obtained the patient's confidence in the first place, and persuaded him that he would get comfortable if he would do exactly as he directed him. There were certain habits which he made him cease, etc. The case of mysophobia which he saw was improving when he made his visit, and he advised the using of a little stronger moral efforts than had been used, and he thought the patient would ultimately get comparatively well.

The Secretary then read by title a paper on "Mental Physics," by S. V. Clevenger, M.D., of Chicago; and a paper entitled "Note, with Seven Photographs, on the Impossibility of Mistaking the Auditory for the Trigeminal Region in the Medulla Oblongata of Reptiles," by John J. Mason, M.D., of Newport,—the accompanying illustrations, as shown, demonstrating the point of the paper perfectly.

Dr. G. BETTON MASSEY, of Philadelphia, then read a paper in which he reported "A Case of Sudden Loss of Vision Following Anæsthesia of the Fifth Nerve, with Remarks on the Modifying Effects of Anæsthesia on the Galvanic Reactions of the Special Senses." R. F. A., forty years of age, a farmer, muscular, well developed, and healthy, married three years ago, never drank to excess nor had syphilis, with no history of venereal abuse nor loss of venereal power, was subject to headache, had always used tobacco freely—at times excessively.

Three years ago, while hiving honey bees, he was stung on the left cheek, and was astonished to find that it gave him no pain. From this he noticed that the left side of his face was quite numb. This numbness increased steadily for about two years, and was accompanied by heaviness and pain felt mostly on the left side, though at times extending over the whole head. At the end of two years he noticed that the sight of the left eye was impaired. This rapidly grew worse, and in ten days vision was totally lost, not even light-perception remaining. At this time he came under the care of Dr. William Thomson, from whose notes Dr. Massey condensed the account of his visual condition. Vision of the right eye $\frac{6}{6}$. The left eye blind, and diverges. Both pupils small and immovable. Inspection otherwise normal. Tension normal. No glaucoma. The third pair was not involved. Has adduction when the right eye is closed. The divergence is not due to paralysis of the internal rectus muscle. Ophthalmoscopic examination of the left eye showed white atrophy, the disk being snow-white. Dr. Massey then gave a detailed account of the history of the case as seen one year subsequently, when over the left eye there was found a partially anæsthetic spot, about four centimetres long by three wide. A needle thrust entirely through the skin, although felt by the patient, did not produce pain. The anæsthesia gradually lessened as the periphery of the area was approached. Sensation to the faradic current was lessened. The local galvanic sensations presented a decided deviation from the normal. The sensation of burning was normal. The sensation of tingling was about two thirds lessened. The more distant effects of the current were also about two thirds lessened. Vertigo was readily produced with the poles placed anywhere except in the anæsthetic area, where thirty-five cells were required, while but twelve sufficed for the opposite side of the forehead. No sensations of light could be produced in either eye by the strongest current within the endurance of the patient. Fearing that the remarkable lessening of normal vertigo and taste originally found in the case might be due to the increased resistance as to the anæsthetic spot, the amount of current actually passing through the circuit was carefully estimated by the deflection of the galvanometric needle. The difference in the results on the two sides could only be explained by placing these phenomena in the already large list of reflex actions, the integrity of one side of the arc in this case being impaired with a corresponding impairment of the phenomena. This solution of the "electric taste" and the "electric flash" was suggested some years ago by Althaus, in his work on electro-therapeutics. The importance of the case here related consists in the clear relation established between the restoration of sensation and the return of the phenomena.

Remarks on Dr. Massey's Paper.

Dr. ROCKWELL, of New York, said that the paper had opened up the question of the effect of electricity upon the special senses. It was a well-known fact that when electricity was passed longitudinally through the brain it caused no dizziness, but when passed transversely it did, and the explanation is in the fact that the brain is unsymmetrically affected, and dizziness is the result. The effect of electricity upon the sensation of taste is remarkable. He had had many cases of loss of taste, many cases where both taste and smell were entirely lost, and it was a singular fact that when taste and smell were lost, the galvanic current would not produce the slightest metallic taste; it·is only when this sensibility returns that the sensation of taste returns.

Dr. GEORGE W. JACOBY of New York, then read a paper on "Cerebro-Spinal Saturnism," in which he reported two cases, one of *saturnine paraplegia* and one of *saturnine ataxia.* It was with a decided purpose that he made use of the above title, and did not employ the term which had been recommended by most writers, namely, that of encephalopathy, which had been used to designate spinal symptoms as the result of lead-poisoning. Dr. Jacoby thought he was warranted in concluding that Falk had never seen such cases as formed the basis of his paper. Jaccoud was the first to distinctly oppose the term encephalopathy, which he did in 1867, and in the same paper described a case of saturnine paraplegia, and other symptoms which might occur as a result of lead-poisoning. Ataxic symptoms were not mentioned. After reviewing the literature of the subject of saturnine ataxia, Dr. Jacoby gave the history of two cases which had been under his observation, and to which he gave the names saturnine paraplegia and saturnine ataxia. In the first case there was no difficulty in making the diagnosis of saturnine paraplegia. The history of preceding forms of lead-poisoning and the development of brain symptoms quite suddenly, together with the spinal ones, both of which occurred without any rise of temperature, together with the extensor paralysis of the hands and the blue discoloration along the gums, made the diagnosis an easy one.

The second case, one of saturnine ataxia, presented greater difficulties of diagnosis. First, because of its rarity; and, second, on account of the patient having had syphilis. The pupils were equal, the temperature and pulse normal; he began to experience difficulty in using his hands, they were clumsy, he had trouble in buttoning his clothes, could not use his knife and fork; finally was in a condition of almost utter helplessness, which became complete when he noticed that his feet were also becoming affected. He did not feel the floor under his feet, he staggered when he walked or stood, ultimately was obliged to remain seated or to crawl around on the hands and knees in order to keep from falling. When Dr. Jacoby saw the patient there was no paralysis or loss of muscular power in the lower extremities, which presented no changes discernible to the eye. The patient could, while lying on his back, freely execute all movements, and develop a normal amount of force. At the same time, there was complete anæsthesia of the entire extremities. When started he could walk, but threw his legs forward, lifted his feet high from the ground and brought them down with the heel first. Entire absence of patellar tendon reflex. The facts that influenced him in making the diagnosis were, first, the symmetrical bilaterial paralysis of the extensors of the two middle fingers of each hand; second, the decided spinal symptoms coming on after the cerebral ones; third, the history of the attack and prodromal symptoms. The fact of the patient not having had any blue lines on the gums, nor any colic, did not invalidate the diagnosis in the least.

If his paper was the means of attracting more general attention to this class of cases, and, through having shown their rarity, he could induce physicians to publish any case that might come under their observation, or might even now be hidden in their case-books, it would have served its purpose.

Remarks on Dr. Jacoby's Paper.

Dr. AMIDON, of New York, remarked that he was unable to see why there could be any more doubt concerning the power of lead to produce ataxia than there was concerning other metals, or poisons, such as arsenic and the diphtheritic poison, both of which were known to produce ataxia and to present symptoms similar to those related in Dr. Jacoby's case.

Dr. JACOBY.—" But is it not strange that these cases have not been reported?" He had not been able to find any reference to them, except two cases in Reynold's system of medicine.

Dr. BIRDSALL said he had observed very marked tremor in cases of lead-poisoning, which even approached the ataxic form; so much so that it was often difficult to distinguish whether or not slight ataxia was not combined with tremor; but he had not seen ataxia confined to the lower extremities in saturnism.

Dr. DANA remarked that it seemed to him we could agree that lead-poisoning was extremely common without ataxia, since within the last ten or twelve years a very large number of cases of lead-paralysis had been reported, and in none of them had mention been made of ataxia. It would seem to him that it would require a great deal of evidence to prove that it was ataxia in Dr. Jacoby's case due to lead-poisoning.

Dr. MASSEY, of Philadelphia, had seen a great many cases of lead-paralysis, but none with ataxic symptoms.

The committee appointed to report a letter or minute on the death of Dr. George M. Beard presented the following report, which was accepted, and the committee was discharged with the thanks of the Association:

The committee to whom was referred the matter of the resolutions regarding the death of Dr. George M. Beard, request that the Secretary record in the minutes of the Society an expression of deep regret on the part of the Society at the loss of a valued fellow-member, and an expression of high appreciation of Dr. Beard's original talents, his persistent industry in scientific work, and his important contributions to neurology and psychology.

C. L. DANA, M.D.
C. K. MILLS, M.D.
Committee.

Dr. AMIDON moved that the Association adjourn, to meet in 1885, subject to the call of the Council with reference to time and place. Carried.